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AN INVESTIGATION OF THE TECHNIQUE OF PSYCHO-ANALYSIS

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PREFATORY NOTE

ON BEING APPOINTED Director of Research to the Institute of Psycho-analysis my first official activities were directed towards organising psycho-analytical work on the psychoses. With the help of those interested in this subject a scheme was drawn up, but so far the work done has not justified the publication of a report. My next and more ambitious project was to systematise and correlate work on the technique of psycho-analysis. Having collected a series of typical problems I decided to approach the subject by the Questionnaire method. This method is no doubt a tedious one and subject to many disadvantages; but it has the virtue of giving adequate representation to a solid body of opinion which is seldom or never expressed otherwise. The results are now published in report form. It was found necessary to add a running editorial commentary on the various sections and subsections. It is hoped that by adopting this method, not only will a more coherent and systematic impression be given, but problems requiring further investigation will be made to stand out more clearly. It should be added that, in order to eliminate as far as possible the factor of subjective bias, the replies to the Questionnaire were first of all sifted and summarised by Dr. Marjorie Brierley. This by no means easy task was carried out by her with considerable

skill and a reassuring measure of scientific detachment. Notes of discussions were kept and after putting the whole work aside for a year or two I approached the subject afresh. The ideas that suggested themselves were then systematised and subsequently checked for error by Dr. Brierley. Although Dr. Brierley is in agreement with them, I feel it desirable to point out that the responsibility for this commentary is mine. There is, of course, no suggestion that these, or indeed any of the other opinions quoted throughout the text, represent the last word on the subject. With the lapse of time new problems inevitably arise and even established views change. Periodic revision of technical principles is obviously essential for the good health of any science, and I look forward to a renewal of this investigation in the near future.

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CHAPTER I

INTRODUCTORY

General Considerations. As the literature of psycho-analysis expands it becomes increasingly obvious that problems of technique are not given the amount of open and sustained discussion their importance warrants. This does not mean that questions of technique are neglected by analysts. On the contrary, quite active discussion of technical problems goes on. But it takes place as a rule between individuals or in small private groups, less frequently in scientific meetings, and still more rarely in the pages of scientific journals. A notable exception is the *International Journal of Psycho-analysis*, which has published a number of articles and two set courses of lectures on the subject. These contributions were of necessity extremely condensed and could not pretend to satisfy the needs of the practising analyst or candidate.

This absence of open discussion is interesting in two respects. In the first place it suggests that there are intrinsic difficulties in the way of describing and systematising psycho-analytic technique. Alternatively it is possible that subjective anxieties concerning psycho-analysis tend to focus on technique and give rise to inertia in open discussion. This seems all the more likely in that so much technical discussion centres round the phenomena of transference and

counter-transference both positive and negative. Moreover, the mere possibility that an anxiety factor may influence discussion of technique makes it all the more necessary to arrange frequent symposia on the subject when those qualified to take part can exchange views with complete freedom. Private discussions between individuals are useful and inevitable, but unofficial group discussion has several drawbacks, not the least of which is that it tends to foster eclecticism. Eclecticism and anxiety are questionable scientific travelling companions. Their combined influence is disruptive.

With regard to the first possibility, viz. the existence of intrinsic obstacles to describing or standardising technical procedure, little difference of opinion is likely to arise. It is reflected not only in the wish frequently expressed by candidates that they could " listen in " to analytical sessions conducted by more experienced colleagues but also in the suggestions made by critics that there can be no adequate scientific control of psycho-analysis until, for example, dictaphones or microphones are installed in the consulting-room. This last consideration suggests that the obstacles in the way of obtaining and imparting exact information as to procedure constitute a special set of factors which perhaps more than any other contribute to difficulties (more accurately, differences) in technical discussion. In the first place, apart from some very illuminating articles on technique and numerous references scattered throughout his works, Freud never embarked on a systematic *descriptive* account of the technique of his own analyses. Secondly, the handing-on of Freudian technique has been undertaken, not

only by his own professional analysands but also by pupils who absorbed his teaching in less direct ways, i.e. who had no personal experience of Freud's own technique. And these less-directly instructed pupils have in their turn handed on their own methods to fresh generations. Thirdly, although it may be assumed that the training analysis of professional pupils can remove most of the cruder forms of " transference-neurosis," there is plenty of evidence that candidates are left with residual transferences which express themselves in a characteristic professional form. When these are of a mildly positive nature the candidate may be depended upon to copy and support the analytic procedures he has himself experienced. When, however, they are of a negative type a variety of reactions may ensue. The candidate may ape his analyst's technique to the point of caricature or go out of his way to defend it, sometimes with an obvious degree of emotional heat. Or again he may make a point of adding to or correcting that technique in a way that alters its character considerably. All such reactions might well be called " training transferences." Their most paralysing form is, no doubt, the habit of slavishly copying the training analyst's technique whether it is suitable for the case or not. Moreover, there is no guarantee that prolonged analyses will eliminate training transferences. On the contrary, they may well foster them. When analyses exceed an optimum duration the candidate must feel more and more committed to the system adopted by his analyst. At the least it would be difficult for the candidate to assume that there were any flaws in the method. In short, it would appear that psycho-analysts are

more afraid than other scientists to create an impression of " transgressing." No doubt there was a time in the history of psycho-analysis when criticism of principles or practices was only a preliminary to complete repudiation of them. And although the basis of psycho-analysis is now unshakable, the expression of unorthodox views may still be suspect. Not only so ; there may still be a degree of moralistic satisfaction to be obtained from expressing orthodox views. When the number of analysts increases and there is less likelihood of positive or negative personal reactions developing between them, these factors will no doubt diminish.

But at present they combine to foster powerful and not always unobtrusive *traditions*. And since the sources of these traditions vary, there is likely sooner or later to be some clashing between them. Before such ultimate clashes occur, there is always a phase of uncertainty, and this in former years was commonly expressed in the question : do you know what Freud himself would do under such-and-such circumstances ? It might even be profitable to trace the genealogical tree of a number of analysts and then submit to them some test problems in technique in order to discover how far transference traditions influence the adoption or rejection of technical devices.

The second consideration, viz. that factors of anxiety and guilt may influence technical discussion, is also beyond dispute. These reactions are in some respects less marked between candidates who have more frequent opportunities for unhampered discussion. Analysts whose training was acquired before the existence of training institutes, who in fact trained

themselves by the system of trial and error, are likely to be more sensitive. The two most obvious manifestations of this reaction are, first, the existence of timidity and inferiority feeling during actual discussions, and, second, the existence of superiority reactions and marked inclinations to perfectionism on matters concerning which a more empirical not to say modest attitude of professional interest is desirable.

A serious objection to this combination of superiority and inferiority reactions on technical matters is that it tends to lead to discharge in the form of hearsay reports. The most inveterate gossips about psychoanalysis are naturally analytic patients, although non-analytical colleagues come in a very good second. But analysts themselves are not immune to the habit, which in their case takes the form of a garbled report on the technique of some other analyst. The gravamen of the charge naturally varies with the technical fashion of the period, but the tendency to criticism remains constant, is indeed enhanced by the difficulties of technical discussion. The analyst who in confidential moments imparts the information that " So-and-so never analyses the negative transference " (or " deep anxiety " or " aggression ") implies that his own procedure is the only laudable one, an attitude which is scarcely calculated to promote freedom in scientific discussion. Apart from all this, the existing system of " controlling " the candidate's cases makes correlation of technical method a matter of urgency. Systems of control vary in many respects, but in particular they vary in accordance with the individuality of the control analyst and the methods he follows in his own practice. Like all other teachers, control analysts have individual

systems of instruction, and when in addition there are actual differences in analytical outlook and practice, candidates may either become confused or tend to follow too slavishly the individual methods they have been taught. The same is true of all medical teaching, but as psycho-analytic instruction corresponds more closely to the old " apprentice " system, there is not the same possibility of controlling error in method.

It is sometimes argued that even if control analysts unwittingly influence candidates to adopt special procedures they may nevertheless correct errors due to transferences to the training analyst. And certainly it should be the duty of those who control the candidate's clinical work to point out bias due to the influence of the training analysis. Unfortunately the system is far from foolproof. In the first place, the transferences existing between the candidate and his control analyst are much weaker than those between the candidate and his training analyst. Secondly, concealed negative transferences to the training analyst can be easily displaced to the control analyst. And, lastly, if the control analyst happens to have the same clinical and theoretical views as the training analyst, it is more than probable that the duty of correcting bias will be carried out in a perfunctory manner.

In short the conclusion is irresistible that wholesale uncritical adoption by the candidate of the training analyst's methods is not a very desirable state of affairs. It is a childlike reaction that can often be detected in a more exaggerated form in those patients who love to mimic or caricature the mental habits of their analysts. And it was justifiable only during that early phase in the development of psycho-analysis when the

communication of analytic procedures was in the hands of a few individuals and when there was some risk that the methods might be watered down because of internal resistances. Moreover, technique has to be adapted not only to individual patients but also to individual practitioners. Thus while training must aim at providing the candidate with a technique based on scientific principles and adequately flexible in respect of variation in his clinical material, it should also inspire him to find and to adopt such methods of procedure as are consonant with his own personal character and abilities. Transference traditions may sit well or ill upon their wearers, but the most serviceable techniques are probably those moulded to the measure of those who use them.

For these and other reasons there seems ample justification for attempting to pool technical ideas more thoroughly.

Collection of Data. In the early days of psychoanalysis collection of data was largely an empirical exercise. Theory had not yet reached the stage of organisation at which it could be employed to further technical procedure. In recent years, however, the empirical approach has to some extent given place to a description and regulation of technique in terms of existing accepted theory. This method is inevitable, but has some drawbacks. It assumes that existing theory is commonly accepted in its entirety, which is far from being the case. For example, the interpretation of castration anxiety varies in accordance with the analyst's preconceptions. It may be regarded as a characteristic product of the " phallic " phase of development or as a late derivative of primary infantile

anxieties in which the libidinal element is to some extent of secondary significance. In the next place, the approach through theory tends to slow up research. As soon as a control analyst says " This is right " or " That is wrong," instead of " This or that is expedient," he has not only opened the door to a form of " technical morality " but has frightened his student off any spontaneous tendencies to research.

It would seem desirable, therefore, to abandon for the time being the theoretical approach and make a fresh empirical study of the subject. And the simplest way of doing so seems to be to ask individual analysts the direct question, What do you actually do in analysis ? Following this plan a Questionnaire was drawn up and sent, in the first place, to those members of the British Psycho-analytical Society who are engaged in active psycho-analytic practice.

Form of Questionnaire. There are two simple methods of drawing up a Questionnaire on psycho-analytic technique. The more systematic is to arrange the queries in accordance with a preconceived outline, touching on the most important aspects in a definite sequence. One might, for instance, arrange questions chronologically, with regard to problems arising in the opening, intermediate, and terminal phases of an analysis ; one might investigate the problems in terms of topography and structure, classifying them according to systems *Cs*, *Pcs*, or *Ucs*, or ego, super-ego, and id ; or one might adopt a system based on case-differentiation, technique in hysteria, obsessional neurosis, psychoses, etc. The alternative method is to ignore systematic arrangements and restrict oneself to problems most frequently discussed by individual

analysts and students in the process of clinical training.

After due consideration it was decided to adopt this second more empirical approach. For some years now notes have been made of questions on technique asked not only by candidates but by analysts of varying experience. Scrutiny of a wide range of such questions showed that they could be roughly divided into groups according to certain characteristics of the questioners :

(*a*) in accordance with the date they first learnt theory ;

(*b*) in accordance with their etiological views ;

(*c*) in accordance with their main clinical interests, e.g. psycho-neurotic or psychotic ;

(*d*) in accordance with their interest in different mental levels. Here there appeared to be two types : (1) the analyst interested chiefly in the deeper layers of the pre-conscious and in material subject to actual repression ; and (2) the analyst interested in the systematic unconscious or dynamic unconscious the content of which has never been pre-conscious ;

(*e*) in accordance with their interest in endopsychic factors or environmental influences. (This last division of interest has existed since the earliest days of analysis, when discussions of the significance of real psychic traumata were extremely lively.)

When it came to arranging these questions in a suitable form, two facts emerged : first, that there was bound to be a good deal of overlapping, and, second, that some matters of importance were comparatively

neglected. For example, it was remarkable how few of the collected problems were directly concerned with "transference" difficulties; still fewer were the questions asked about counter-transference. Nevertheless, it was felt desirable to ignore overlapping and to confine the first questionnaire to those problems that had actually been raised from time to time. In order to rectify possible omissions, analysts were asked to append to their replies a list of special problems which they would wish discussed.

Obviously a certain amount of objectivity and good will had to be taken for granted. Nothing is easier to sabotage than a psychological Questionnaire. Anticipating the possible operation of timidity and guilt factors, a covering appeal was included to the effect that counsels of perfection should be avoided and where practicable an objective description given of common usages.

Representative Nature of Returns. The form was sent to twenty-nine practising analysts and replies were obtained from twenty-four. The absence of five returns might have had the effect of vitiating any findings, but on going into the matter more fully it was found that the analysts in question did not belong to any one group or body of opinion. As regards experience and orientation, they were a representative sample of the whole group. Hence the findings established may be taken as representative of the British Group as a whole up to the year 1938.

Judging by the fact that answers to certain questions were omitted and that in other instances replies were accompanied with explanatory justifications, it appeared that guilt and timidity factors had operated to a

certain extent. But taking it all over, the replies were frankly objective, most obviously so in the case of analysts of long experience, who in fact responded most generously to the appeal. There was little manifest indication of annoyance at the apparently elementary nature of some of the questions, but of course it has to be remembered that the Questionnaire was compiled from problems posed at one time or another by some of the very individuals to whom the form was sent.

Estimation of Results. Owing to the smallness of the numbers involved, it was scarcely worth while attempting to apply formal statistical methods to the material. Nevertheless, using very rough standards, it proved possible to establish the existence of certain general tendencies with or without strong minority opposition. On other questions it became clear that opinion was equally divided. When there was a consensus of opinion in at least two-thirds of the total replies this was regarded as representing a " general habit, tendency, or practice." When the minority amounted to at least one-third of the total, it was assumed that a " strong body of opinion " existed in favour of (or against) the practice in question. Individual opinions strongly expressed were given special consideration.

One obvious objection to this method is the possibility that many " general tendencies " are due to cautiousness, conservatism, or even timidity in those replying to the Questionnaire. An isolated minority opinion might be more important than a large number of replies made on the assumption that no other practice is possible or desirable. On the other hand, provided

one gives adequate expression to minority opinion, there is no harm in proceeding on the assumption that, subject to qualification, certain technical devices appear to be most commonly employed.

Adopting these rough statistical methods, the question then arose, how to present the results in the most effective way. Should one, for example, subdivide the replies in accordance with the age, experience, method of training, clinical interests, or theoretical preconceptions of the analysts concerned? Objections to this course are, in the first place, that the numbers are already too small to make further subdivision of any statistical significance. Secondly, even if one did subdivide replies in accordance with the experience or standing of individuals, there is no guarantee that the results obtained would be any more valuable. As in other sciences, age and experience are no guarantee of increasing wisdom. An analyst of a few years' standing may be able to contribute as much to technical discussions as an analyst of more mature years. Division by methods of training or theoretical preconceptions is likewise unsatisfactory. It does not follow that analysts who began work in the early days of analysis are necessarily " set " in technical habit. Nor does it follow that if a number of analysts trained under more modern conditions share the same views, this consensus is of special significance. It might simply represent a newer form of conservatism.

In this dilemma the best policy seemed to be to fall back on a standard followed with considerable advantage by the medical profession. Although modern " specialism " has to some extent interfered with the system, medical tradition has always granted equality

of opinion to all legally qualified practitioners. And unless the qualifications are to be made fantastically rigid or perfectionist, there seems to be no reasonable alternative to this policy. The findings have therefore been presented in a roughly statistical fashion without attempting any correction for individual differences, but adding wherever necessary what might be called minority reports.

Supplementary Investigations. The results of the Questionnaire were submitted to the British Psychoanalytical Society for discussion. After this a Supplementary Questionnaire was sent out dealing with matters about which more information seemed desirable and raising a number of additional issues omitted in the first Questionnaire. Further discussion took place on the basis of this fresh information. The results of these discussions are included in the report, together with a résumé of certain papers given to the Society. These are included because they deal in some detail with points less exhaustively considered in the Questionnaire.

Arrangement of Report. Copies of the Original and the Supplementary Questionnaire are given in Appendix I as they were sent out. The Report itself is divided into sections and sub-sections (*vide* Contents) dealing with a central topic or group of related topics. Each section includes a summary of the relevant answers given in response to both Questionnaires and comments on these, together with a brief résumé of subsequent discussions and papers having reference to the points at issue. The original numbering of the questions is given in parentheses in the report although their order is necessarily sometimes

changed. Thus (Q.I. (2)) indicates the second part of the first question in the main Questionnaire while (S.Q.C. (6)) is the sixth part of the third Supplementary question. In addition to the Questionnaire forms the appendices include a summary of complete agreements, disagreements, and intermediate results and a review of tendencies displayed in the Society subsequent to the period covered by the report.

CHAPTER II

INTERPRETATION

A. General Methods

It is not surprising that one of the commonest subjects of technical discussion should be the methods of analytic interpretation. But it is interesting to note that the concern of most analysts is not so much with the criteria of interpretation or with the most accurate interpretation of any given situation (series of associations, dream, or symbol) as with the quantity, form, and timing of interpretations. The fact remains, however, that advice is most frequently sought on these matters, e.g. "When do you interpret? How often do you interpret? Do you talk much or little? Do you change your form of interpretation?" A little reflection will show that these questions are not really so trivial as they appear. They do not indicate a substitution of frivolous for more profound analytic interest. Behind them lies anxiety about stereotyping the analytic situation on the side of the analyst. This is an anxiety that exists most commonly in analysts having an obsessional type of mental response. It is easy to observe that all patients tend to stereotype their own analytic responses. Even the apparently sporadic outbreaks of hysterical patients show sooner or later a tendency to rhythm. But obsessional cases make the

most persistent use of this form of protection (resistance through rigidity). Should the analyst on his own side develop a similar reaction or habit, it is clear that a situation of " analytic combat " may arise. Such a possibility is calculated to arouse in the analyst conflict over his own unconscious sadism or over the libidinal symbolism of combat. There are of course more rational aspects of the problem. An attitude of readiness or elasticity on the part of the analyst, in other words, an adaptability to changes or stresses in the analytic situation, is a distinct therapeutic asset. The analyst may well suspect any tendencies on his part to habit formation. On the other hand, partly because of the limited duration of the analytic session, it is difficult to avoid some degree of stereotypy. Anxiety situations frequently arise either at the beginning or at the end of a session, and this fact itself leads to giving interpretations at the same time each day. And as soon as patients have grasped that the analyst has a sort of routine response, they begin either to react against it or to insist that it should be adhered to. Every change in routine is suspected as an act of aggression or as a libidinal advance or as a deprivation. Hence in the long run most of these problems touch on the subject of counter-transference. It is interesting, therefore, to establish what the usual practices are in these matters.

1. **FORM** (Q.I. 1). *Do you prefer :* (1) *short, compact interpretation ; or* (2) *longer explanatory interpretation ; or* (3) *summing-up type,* (a) *trying to convince by tracing development of a theme,* (b) *proving* (*or amplifying*) *by external illustration.*

The replies to this question showed that a majority of the analysts concerned prefer short, compact, *ad hoc* interpretations. That is, the majority use this type most and find it most effective. But a substantial minority (just over one-third) stated a preference for longer explanatory interpretations without precluding the use of short ones. In subsequent discussion it appeared that this divergence in practice turned partly on a difference of opinion as to the limit of utility of short interpretations. Some of the " long " group maintained that the lay-out of an important psychic situation or the transition from one phase to another could not be appreciated until it was clear in all its details (which might take two or three months). Understanding of such situations could not be adequately conveyed to the patient without full coherent explanation, involving usually a certain amount of recapitulation and tracing of themes. But other " short " adherents contended that such situations could be adequately interpreted piecemeal in course of development provided the analyst hit the right nails on the head as they appeared. It was queried whether the aim of long explanations was not directed to reassuring rather than to making the unconscious conscious. It was also suggested that the two types might work differently as regards (*a*) immediate effect, (*b*) influence on progress as a whole, but no decision was arrived at on these lines. The point was raised that the optimum mode of interpretation might vary with clinical types and with individual receptivity.

About one-third of the " long " minority trace themes in order to convince the patient. These consider it helpful, sometimes indispensable, to carry the intellectual faculties of the patient with them as a means of reducing resistance. Others expressly stated that they trace themes only to promote understanding, not to produce conviction, and they doubt whether such conviction has any effect in reducing resistance. Discussion emphasised this difference in estimation of the value of the patient's intellectual co-operation. Between the extremes of those who considered it essential and those who regarded it as either negligible or ineffective there appeared also a moderate section. These maintained that, in general, the

value of such co-operation must vary with the patient's degree of insight, but that refusal of such co-operation might easily mean undesirable and unnecessary frustration. One speaker remarked that in some cases intelligent criticism on the part of the patient was a sign of progress which it would be unwise to crush by refusing discussion. Apart from these differences, it is evident that there is a strong minority in favour of tracing themes to the patient, which involve fairly long explanations. It was also clear that all types of interpretation are used on occasion by all analysts, whatever their preferred methods. Indeed, one of those most strongly in favour of short, compact interpretations for general use expressly stated that she sometimes gives long terminal interpretations (even up to thirty minutes).

Another sharp difference of opinion came to light in connection with the use of external illustrations. The commonest forms of external illustration were drawn from case material, e.g. thumbnail sketches of similar reactions occurring in or interpretations made to other patients, descriptions of standard psycho-analytical observations relevant to the point at issue, reference to similar reactions occurring in children, amplifications of the meaning of symbolism, the free use of analogies, etc. The object of such external illustrations was either to drive a particular interpretation home, or, in some instances, to make an interpretation less " personal " by referring to its validity in other cases. One-third use the method freely, one-third sparingly, while one individual made a special point of never using it. The chief objection to its use appeared to be the risk of interrupting or switching the patient's trend of association by introducing external material or replacing the patient's imagery by the analyst's. The relevance of the analyst's imagery and associations would depend upon the state of rapport existing with the patient at the time. Presumably subjective factors may influence the analyst's willingness to trust his own associations. One member took up the stand that the use of analogy is itself a mode of interpretation.

On the whole the answers to this part of the Questionnaire were satisfactory. It emerged that the short,

compact interpretation was in fairly general favour. There are, of course, a number of general considerations that would support this view. It is in keeping with the principle of psychic economy. Not only is the content of the unconscious relatively simple and compact in form, but there is a general tendency to economy of expression. It is likely, therefore, that the unconscious responds more naturally to short interpretations provided they are to the point. Again, in hysterical cases where faulty repression is one of the main features, it seems likely that a short interpretation is more likely to act as a catalytic agent and promote recovery of memories or catharsis of affect. If this be the case it seems desirable that any rule of short interpretations should take cognisance of the clinical condition of the patient. Obsessional cases would often require more copious interpretation. The symbolic significance attached by the patient to talking should also be taken into account. Many patients resent what they regard as talkativeness on the part of the analyst. Others require a *quid pro quo*: when they have contributed what they feel to be good material they wish to have a return in the form of interpretation and won't be happy till they get it. This state of affairs is no doubt easy to analyse in terms of appropriate genital and pre-genital situations and phantasies and the relevant interpretations should be given. The fact remains, however, that individuals of this type have a characterological or temperamental system which there is no point in frustrating. Hence it should be a universal rule to study the patient's needs in the way of analytic response and to respect them. Provided they are subjected to adequate analysis,

these needs should be met as far as possible until such time as it is unnecessary to do so.

With regard to tracing themes, there is not much doubt that analysts are prone to rediscover in their patients the established findings of psycho-analysis and to enjoy demonstrating their rediscoveries to the patient. In this way they try to resolve their own doubts and their appeal to the intellect of the patient is probably an attempt to allay projected doubt. On the other hand, it is a fact that associative themes do exist, and that where the patient takes kindly to long interpretations, tracing the theme does help to drive the interpretations home. Provided one remembers that short interpretations are always needed, there is no reason one should not give longer " theme " interpretations as well, whenever this is likely to secure the patient's cooperation and not merely to evoke resistances.

The problem of using analogies raises deeper issues. Some analysts are definitely of opinion (Abraham in particular held this view) that in cases of difficult association or " breaks " it is proper for the analyst to allow his own associations to run to the patient's material and to interpret the end result to the patient. This has obvious drawbacks, but of course the same objections could be made to interpretations given on the spur of the moment. These are doubtless the result of a swift, almost automatic reading of the patient's material by the analyst's unconscious or preconscious. The same applies to the use of illustrations and analogies: the selection of the analogies by the analyst is brought about by two factors, the stimulus of the patient's material and the analyst's elaboration of

that material. The use of external illustration or analogy is (or can be) a part of the technique of dosage, i.e. it is possible by this means to give a modified (slightly censored or impersonal) interpretation and thus open the way to a deeper and more direct interpretation. So long as this course is justified by the facts of the case and not merely stimulated by anxiety over giving the deeper interpretation without modification, there seems to be no objection to it.

2. TIMING (Q.I. 2). *What is the favourite point of interpretation?*—(1) *early in session;* (2) *middle or before end* (*allowing a space for elaboration*); (3) *at end: "summing-up" fashion.*

A majority of the replies to this question were non-committal. Most people seemed to have no definite rule as regards timing interpretations in any one session, but varied their timing according to circumstances. Nevertheless, there appeared to be a general tendency to regard the later rather than the earlier part of the session as the most suitable time. A series of psychic events fall into truer perspective towards the end of the session. A majority favoured allowing time for elaboration as against the analyst having the last word. There were very few advocates of routine terminal summaries, some definite objectors. One analyst discriminated between anxiety-evoking interpretations and reassuring ones, and was inclined, if necessary, to give the latter type at the very end, sacrificing the other type if need be. Two others never end with interpretations or round off a session; one likes to do this. There was a definite clash of opinion as to the use of interpretation early in the session. Two never interpret early, many more do so if or when it seems suitable, e.g. when there is an obvious carry-over from the previous day. Some felt it often essential to start a session with interpretation, especially with patients who have difficulty in beginning and with non-cooperative

psychotics. It was queried in discussion whether initial interpretations are not often reassuring manœuvres, e.g. in relation to negative transference anxieties.

It is evident from the replies that although the timing of interpretations is not regarded as of major importance, there is no great objection to the development of a timing habit. Perhaps the subject has not been given adequate consideration by most analysts. It is often taken for granted that it is natural to interpret at a certain point in the session, and no doubt this is true in the general sense that the appearance of resistances is a definite indication for interpretation. And there is no doubt that patients themselves tend to follow a set habit. They may produce their most illuminating material or their most intense resistances towards the end of the session. This is perhaps the most common occurrence. Or they may begin with some minor manifestations of a positive or negative kind ; in the latter instances it would seem a waste of time to delay interpretation. Or, again, they may, as it were, shoot their bolt towards the middle of the session and go on to " play out time." In short, the habit of giving interpretations at a fixed time is to some extent forced on the analyst by the patient. These timing habits on the part of the patient are no doubt characteristic for each case and deserve interpretation on their own account. But the matter does not end there. The possibility of a counter-transference system should be taken into account. Some analysts have described what amounts almost to a system of compulsive interpretation, where a definite urge to give an interpretation is experienced apart from any

consciously observed indications. The whole process is probably much more intuitive than is generally conceded. Anyhow, further investigation of this problem seems to be called for.

3. **AMOUNT** (Q.I. 3). (1) *Throughout the analysis : as a rule, do you talk much or little ?* (2) *Early stages : how long do you usually let patients run without interference ? How soon do you start systematic interpretation ?* (3) *Middle stages : is your interpretation on the whole continuous and systematic or do you return from time to time to the opening system of letting them run ?* (4) *End stages : do you find your interpretative interference becomes incessant ?*

This set of questions was included although at first sight it might appear to overlap with the questions on short or long interpretations. Quite apart from the *length of individual interpretations*, the problems of *when to begin* interpretation and how much interpretation to give in the various *stages* of analysis give rise to a good deal of concern. It seemed desirable to collect information on this specific issue and to discover whether, once interpretation begins, the analyst increases or decreases the dosage during later stages.

A large majority of replies favoured a small amount of interpretation throughout the whole analysis. There were more of those who " talked little " at any stage than of those who preferred short interpretations in individual sessions (see previous section). So it would appear that some who give long explanations nevertheless reported that they do not talk much. This apparent inconsistency may be due to the fact that (as pointed out by some contributors) long explanations need not and cannot be given as frequently as short ones. A few replies

went to the other extreme. They favoured frequent short interpretations every session, the maximum number reported being ten per session.

Practice in early stages of analysis varied. About one-third either interpret from the start or tend to interpret early in analysis. These do not believe in letting opportunities slip simply because they happen to occur early, though they admit that their early interpretations are usually short and sometimes superficial or pre-conscious in type. One or two advised interpreting negative transference as soon as it appeared. About an equal number of replies favoured letting the patient run on for some weeks (about a fortnight was a popular period) unless interference seemed definitely called for. In regard to psychotic patients, one never intervened early, while another was in favour of early transference interpretation with these also.

Practice in the middle stages also showed two opposing tendencies about equally supported. Nearly one half reported that their interpretations became definitely more continuous and systematic in these stages, while the other half intermittently returned to the " free run " expectant attitude. A few stated that their interpretations were always *ad hoc* and never systematic except in the general sense that they " systematically " interpreted manifestations of anxiety. They did not appear to be influenced in this respect by clinical preconceptions, i.e. paying special attention to characteristic mechanisms in different clinical types.

Practice in terminal stages varied considerably. Some replies deplored incessant interference, some found their interpretations tended to diminish in frequency towards the end of treatment, while others found that they increased.

There appeared to be a general tendency to increase the length of interpretations in the middle and later stages. One or two analysts, speaking of " free runs " without regard to any particular stage, said that it did not pay to allow the patient to run without guidance for prolonged periods. There is some issue here as to the amount of working through, or emotional catharsis, which is considered necessary or desirable. Some free runs may be due to the analyst's temporary loss of orientation.

The replies to this section of the Questionnaire were not very satisfactory. It is clear that on the whole those who favour short interpretations also give relatively little interpretation throughout the analysis. But the striking differences of opinion as to when to begin interpretation and how much to give indicate the necessity for further investigation of this subject. No doubt many of these differences are due to variations in the clinical type of case, in the nature or manifestations of anxiety, in the nature or exhibition of transferences. And although the possibility may seem remote it may be that the sex of the analyst influences the degree of activity, the readiness to interfere early, and the amount of interference. But it would be safer to express this last factor in more general terms of counter-transference. With regard to variations in different clinical types of case, there can be no question that this factor should be taken into account by the analyst. Anxiety hysterics usually start easily but some get into difficulties with repressed material and with transferences. This usually involves early interpretation with spells of free running. But such cases are quick to exploit interpretation, which should therefore be sparing in amount and to the point, even if this means frequent repetition. Obsessional cases usually start easily and run easily. More than other psychoneurotic types they tend to resent interference and react negatively (more correctly, in an ambivalent way) to interpretations. This fact should be borne in mind with obsessional types of personality. They resent talkativeness in the analyst and should be given a fairly free run until it is clear that they are developing some signs of the usual obsessional resist-

ance, viz. to turn the analytic situation into an obsessional one. Cases of sexual perversion and inhibition usually require early interpretation. Experience of psychotic cases suggests that the factor of anxiety should be decisive in shaping the policy of interpretation. In depressive cases the transference should also be taken into account. Such individuals are easily " hurt " by silences. Paranoid types require the most patient handling in these respects : they resent talking and yet object to being left to their own devices, an attitude which they construe as hostility or disapproval on the part of the analyst.

Apart from these differences, which could easily be cleared up by a more exhaustive investigation and collection of information, there are evidently differences in principle that call for resolution. Perhaps a supplementary questionnaire on the differences in technique between child analysis and the analysis of adults might help to clarify the principles involved. For example, a preference for so-called " deep " interpretations (see next section), using " deep " in the developmental and topographic senses, would lead naturally to early and possibly frequent interpretation. For if the links between the pathogenic material and the preconscious system have never been well established and if the situation is otherwise satisfactory, there would seem to be little point in waiting and less point in avoiding repetition. It is possible that the practice of child analysis fosters tendencies in these directions, whilst practice of adult analysis leads to giving more consideration to pre-conscious levels. Curiously enough the same differences in tendency are exhibited by organic physicians in their handling of case histories.

Physicians dealing with adult cases pay much more attention to the patient's " story," whereas infants and children are lucky to have any attention at all paid to their (pre-)conscious impressions.

Reviewing all these general aspects of interpretative technique, it is difficult to avoid the impression that many of the differences observed are due to a special factor mentioned in the Introduction, viz. the existence of " training transferences " and traditions. It has not been possible to investigate the matter further, but the answers to the questions seem to indicate that a body of tradition in favour of rather passive and expectant analysis does exist : that this is derived from Freud's own practice, but that it is subject to factors of exaggeration or correction as it filters from one training generation to another. It could be said with some fairness that a majority of those who came under Freud's direct influence are inclined to use expectant methods. But the impression is naturally subject to correction and is given for what it is worth.

4. DEPTH (Q.I. 4). (1) *This can be thought of in terms of degree of repression, conscious accessibility and readiness, or in terms of stages,* e.g. *pregenital as compared with genital interpretation, etc.* Please state individual definition.

(2) *Do you have a standard level of deep interpretation for all cases, or do you have an optimum depth varying for clinical conditions,* e.g. *in* (*a*) *anxiety,* (*b*) *obsessional,* (*c*) *characterological,* (*d*) *psychotic,* (*e*) *normal cases ?*

(3) *On the whole, do you favour deep interpretation in early, middle, or late stages ?*

(4) *Do you favour deep interpretation as the ideal*

criterion or deep interpretation in terms of the reality circumstances : (1) *infancy ;* (2) *childhood ;* (3) *puberty ;* (4) *adolescence ;* (5) *adult life ?*

A few defined depth in terms of stages or developmental levels. Thus one said that pregenital is deep and another that earliest pregenital, particularly oral, is deep. A good half defined it as the degree of repression or inaccessibility to consciousness. Samples of this type of definition follow: " motivation in infantile versus adult terms " ; " what hurts most " ; " deep when the infantile traumata and phantasies are reconstructed." One stated that depth implies a combination of three factors, i.e. pregenital material which has never been pre-conscious and is urgent in the sense of causing anxiety. A few held that depth is really a superfluous term, since immediate transference interpretation is deepest in the sense of urgency of anxiety, and this immediacy or urgency of anxiety necessarily varies in relation to historical or topographical depth. One asked whether the term " deep " has any definite meaning.

One-third reported that they have no standard level of interpretation. Of these one thinks the optimum depth varies with the level to which the patient is fixated, another reaches all levels in all cases at different times. Two others in this group stressed the factor of acceptability, viz. " Take accessible conflicts first " and " Never give an entirely unacceptable interpretation."

A few have a standard aim or goal, e.g. one " deep in all cases." Another varies depth in accordance with the patient's reaction, readiness, and stage of analysis, but considers " deep " the optimum level for all if achievable. Yet another aims at recovering the memories associated with the pathological phantasies. Only one has a standard level for all cases without reference to stages or clinical type, while four stress the variation of the optimum level with the patient. One says, " Interpretation must extend just beyond the ego boundary, but not too far beyond, i.e. progress from superficial downwards."

Opinions varied considerably about the timing of " deep " interpretations. Only one reported the habitual use of " deep " interpretation early ; but a few others give them fairly

frequently, though not usually so " deep " as at later stages. A few definitely prefer to give them late, except under special conditions, e.g. anxiety cases. Most of the others tend habitually to middle or middle and late, with the reservation that they may give " deep " early if indications seem positive. Only three adapt their practice to the variation in insight of the patients and by accessibility to consciousness.

There appeared to be a general feeling that phantasy interpretation alone is inadequate. A majority favoured the correlation of phantasy with reality, ideally with reality of all stages of life, but with particular stress on correlation with events of infancy and childhood. Four considered the recovery of memories of major importance. One liked the relations of super ego, ego, and id factors to reality all to be made plain.

The answers to the questions about " depth " were the least satisfactory of all that were returned. This was due partly to the unclear way in which they were presented in the Questionnaire. For some time previously there had been a good deal of loose talk about " depth " of interpretation, and since the term itself had never been accurately defined, many of the ideas that gained currency were also very confused. In particular, it appeared that an artificial distinction might be drawn between a " deep " school and all others. It seemed, therefore, highly desirable to reach an accurate definition, and on the basis of such a definition to establish what was the common usage of " deep " interpretation. The tentative suggestions included in the Questionnaire were intended merely to stimulate more accurate definition. Unfortunately the replies given did not dispel the obscurity in which the whole subject was wrapped. It may be noted, however, that they tended to define " depth " in terms *either* of developmental level *or* of degree of repression.

This tendency of itself suggests a good reason for dissatisfaction with the term. " Deep " is a word that belongs in the first instance to the sphere of topographic description. It conveys most readily a spatial notion of the Ucs existing below the level of Pcs and Cs (as expressed by Freud in his classical ice-berg metaphor). So long as the Ucs was equated with the repressed, spatial description was not ambiguous. On the contrary, it conveyed a straightforward idea that the lowest level of the Ucs would consist of the earliest " repressed." The ambiguity which has crept in is due partly to the concept of the id (" it is still true that all that is repressed is Ucs, but not that the whole Ucs is repressed " *) and partly to the recognition of defence mechanisms other than repression. Admittedly, the historically " early " often is heavily repressed, but the two would be synonymous only if repression were the sole mode of defence operative in infancy. The either-or definitions imply that the term " deep " does not fit modern concepts easily because it belongs to an earlier phase of theory. Actually this central issue, namely whether there is any object in using the term " deep " at all, was focussed in a paper by James Strachey. The outline of interpretation given by him suggests a way out of the dilemma, and a brief résumé of it is therefore given here.

At a meeting of the British Psycho-analytical Society on June 13, 1933, James Strachey read a paper entitled " The Nature of the Therapeutic Action of Psycho-analysis."† He defined: " One particular sort of interpretation, which seems

* Freud, *The Ego and the Id*, 1927, Hogarth Press, p. 17.

† *International Journal of Psycho-Analysis*, 1934, vol. XV, pp. 127–59.

to me to be actually the ultimate instrument of psychoanalytic therapy and to which for convenience I shall give the name of ' mutative ' interpretation. . . . I shall take as an instance the interpretation of a hostile impulse. By virtue of his power (his strictly limited power) as auxiliary super ego, the analyst gives permission for a certain small quantity of the patient's id-energy (in one instance, in the form of an aggressive impulse) to become conscious. Since the analyst is also, from the nature of things, the object of the patient's id-impulses, the quantity of these impulses which is now released into consciousness will become consciously directed towards the analyst. This is the critical point. If all goes well, the patient's ego will become aware of the contrast between the aggressive character of his feelings and the real nature of the analyst, who does not behave like the patient's " good " or " bad " archaic objects. The patient, that is to say, will become aware of a distinction between his archaic phantasy object and the real external object. The interpretation has now become a mutative one, since it has produced a breach in the neurotic vicious circle. For the patient, having become aware of the lack of aggressiveness in the real external object, will be able to diminish his own aggressiveness; the new object which he introjects will be less aggressive, and consequently the aggressiveness of his super ego will also be diminished. As a further corollary to these events, and simultaneously with them, the patient will obtain access to the infantile material which is being re-experienced by him in his relation to the analyst. . . . First, then, there is the phase in which the patient becomes conscious of a particular quantity of id-energy as being directed towards the analyst; and secondly there is the phase in which the patient becomes aware that this id-energy is directed towards an archaic phantasy object and not towards a real one " (pp. 142–3). Further, mutative interpretations are " immediate ": " A mutative interpretation can only be applied to an id-impulse which is actually in a state of cathexis " (p. 149). Also, " A mutative interpretation must be ' specific,' that is to say, detailed and concrete " (p. 151). Mutative interpretation is " transference interpreta-

tion at the point of urgency " (p. 156). " If I may take an analogy from trench warfare, the acceptance of a transference interpretation corresponds to the capture of a key position, while the extra-transference interpretations correspond to the general advance and to the consolidation of a fresh line which are made possible by the capture of a key position. But when this general advance goes beyond a certain point, there will be another check and the capture of a further key position will be necessary before progress can be resumed. An oscillation of this kind between transference and extra-transference interpretations will represent the normal course of events in an analysis " (p. 158).

In subsequent discussion Ernest Jones felt the author's attitude to extra-transference interpretation to be rather too nihilistic. He maintained that, especially in the early stages of analysis, the emerging id-impulses may be really directed to people other than the analyst and suggested that in such circumstances non-transference interpretations may also be mutative. Melanie Klein said that a mutative interpretation makes one step in the analysis of ego, super-ego, and id complete, provided that the interpretation is really complete. In her opinion it is usually lack of interpretation, not too much interpretation, which causes trouble in analysis. Joan Riviere agreed that extra-transference interpretations lead up to and consolidate transference situations, but she thought they may also bring to light fresh aspects of the transference relation, hitherto undiscovered. She agreed, however, that since transference effects a crystallisation of impulse, the attitude to the analyst must be the core and kernel of every analytic situation, though it may not become clear at once. Other speakers, while paying tribute to the excellence of the paper, did not feel that it had solved all the problems of cure. Some felt that it did not take adequate account of the factor of reprojection.

It is clear that if we accept Mr. Strachey's view there is no point in using the term " deep," since the immediate focus of conflict round which all resistances cluster lies in all cases in the transference. It would

greatly simplify things if we could accept this view. And it has the advantage of being more or less in keeping with earlier pronouncements as to the development of a " transference-neurosis." Writing on this subject some years before,* Glover described the free " floating transferences " that operate at the beginning of an analysis. He added that from the time these floating transferences develop into the " transference-neurosis " everything that happens in every analytic session can be interpreted, *if need be*, in terms of the transference. He did not, however, exclude the therapeutic effect of " extra-transference " interpretations at any stage in the analysis. Also it may be argued that the classical " transference-neurosis " is exhibited chiefly in cases of true psycho-neurosis. Moreover, the theory of " transference-neurosis " was itself based on an earlier mental topography. The working of transference was explained in terms of one mechanism only. Thus displacement was regarded as the essence of transference. Even if we were to include as auxiliaries repression and some degree of projection, that would not bring the transference theory up to date. A modern conception of transference would include not only the *displacement* of some repressed energies or situations but also the re-enactment in the analysis of every conceivable psychic situation, involving the active repetition of *various combinations* of unconscious mechanisms. We know that not all unconscious mechanisms are as amenable to transference modification as displacement. For example, although massive projections on to the image of the analyst do take place, it is not so likely that the

* Edward Glover, *The Technique of Psycho-analysis*, 1928.

mechanism of projection will be limited to the analytic situation. Whereas displacement to analysis can take place, as it were, at first hand, projections must be aided by object displacement before they become true transferences. Similarly with introjections. The very rigidity of introjections and projections, i.e. their comparative refractoriness to analysis, may be accounted for by the fact that a number of extra-analytical outlets are constantly employed. If this be so it would be a mistake in tactics to depend for therapeutic effect solely on transference interpretation. In fact, it is difficult to imagine how " escape " projections (i.e. projections to the external world that ought to come into the analysis) can be guided into the analysis if they are not analysed in their immediate context. In any case, it is clear from the replies that by no means all analysts are ready to accept the Strachey theory unreservedly, and as long as this is the case controversies over deep interpretation are bound to continue.

As has been pointed out, the main obstacle in this discussion is terminological. " Depth " is first of all a topographic concept. But it has also dynamic associations: " deep " can apply to impulses (i.e. of primitive origin) and to affects. Every one of these approaches bristles with unsolved and controversial problems, e.g. the etiology of various psychopathological states; the significance and interrelationship of various developmental " layers," " positions," or " nucleations "; the relative importance of various mechanisms in the total defence system of the mind; the relative importance of endopsychic and environmental factors, of real and phantastic objects, of " objects in the ego "; the nature and relation of

primary and secondary affects, etc. In view of these facts there would appear to be only two legitimate applications of the word " deep." Reverting to an older viewpoint we could use the term as a loose equivalent for the *Ucs* in its dynamic sense (whereby the latter is distinguished from the *Pcs*). It would then follow that, however deep some layers of the *Pcs* may be, the true *Ucs* is deeper still. Alternatively we could divide infantile life into age or developmental periods and assume that " deep " refers to any psychic content deriving from the first two and a half years of life " Deep " transference would then refer to the displacement to the analysis or repetition in the analysis of such content. Better still we might discontinue using the term until such time as all the problems involved are cleared up. Whatever course is adopted it is undesirable that any attempt should be made to differentiate analytic groups on the strength of a word that has not yet been satisfactorily defined.*

* At a Discussion on October 19, 1937, the matter came up again and this time a consensus of opinion emerged, roughly to the following effect: that the term " deep " is so inherently ambiguous that it is better avoided. If used it should be accompanied by an explanation of the precise meaning intended. Restriction to developmenta depth and the " unconscious " nature of phantasy is desirable.

CHAPTER III

INTERPRETATION—II

ANXIETY

AS DISCUSSION OF " deep " interpretation continued it became obvious that some other important issues were involved. The first was the relation of interpretation to the anxieties of the patient. This might be expressed in question form as follows, What are the appropriate means of dealing with these anxieties? It was clear that those who favoured " deep " interpretations (whether they defined the term or not) felt that this method was the best means of dealing with anxieties. On the other hand, those who felt uncertain about giving early " deep " interpretations (whatever they understood by this term) favoured giving more gradual interpretations. Yet another group felt that during phases of anxiety some amount of psychic reassurance was necessary. This might be combined with interpretations (of whatever depth) or followed by them. At this point discussion became more lively. Its tenour was roughly as follows. The " deep " group obviously felt that not only was reassurance ineffective as an isolated policy but that, however accurate it might be, any interpretation that was not " deep " differed very little from reassurance. To which those in favour of reassurance might legitimately retort that the same could be said of " deep " inter-

pretation provided the anxiety actually belonged to some other level. According to this view the reassuring effect of " deep " interpretation might be due to its inaccuracy, in which case the " deep " interpretation would have the same therapeutic effect as a suggestion.

Those in favour of more gradual interpretation no doubt felt themselves open to charges of timidity in handling difficulties, but defended their policy on the score of appropriate dosage. But the issues were much more confused than this outline suggests, and it became necessary to take fresh soundings of the problem in a Supplementary Questionnaire. Additional information was asked for on the relation of " deep " interpretation to reassurance. When the replies came in it was found that the issue of " deep " interpretation *versus* reassurance had given place to a more general concern with the problem of anxiety. In the first Questionnaire this problem had been considered under the following heading :

1. METHOD OF DEALING WITH ANXIETY (Q. 2)
What is your favourite method of dealing with this, e.g. *by rapid interpretation of* (*a*) " *repressed* " *content ;* (*b*) " *repressing* " *factors ; or* (*c*) *by slower expansion of the emotional state, combined with a degree of reassurance* (*postponing deep interpretation till later*) *?*

A large proportion of answers favoured rapid interpretation in states of *acute anxiety*. A majority interpret both " repressed " and " repressing " factors, but with bias in favour of " repressing." One analyst avoids rapid interpretation even in acute states. A minority were of opinion that the essential

interpretation is that of the " repressed " provided it is " urgent." Another view was that interpretation of both ' repressed " and " repressing " should be in terms of anxiety, i.e. urgency. (It has been noticed in control work that candidates often justify interpretations by saying that the patient was in a state of great anxiety.) It was also stated that immediate interpretations of both " repressed " and " repressing " factors must be in transference terms. One " specific " suggested was interpretation of aggression-guilt towards the analyst.

Where anxiety is not acute a great majority prefer slow expansion of the emotional state with postponement of interpretation, and a moderate body of opinion favoured reassurance. One person definitely avoids reassurance on principle. A few stress the factor of transference hostility. One believes in continued interpretation of the danger that is *most* feared.

2. NUMBER OF ANXIETIES (S.Q. D(10)). *Do you open up anxieties from different sources simultaneously or do you concentrate on one main source at a time?*

Answers showed a considerable range of opinion. A small majority favour concentration, but some of these say that though t is better to clear up one focus at a time, it is not always possible. A minority open up different sources simultaneously, while others do this if it seems indicated. They take what the patient offers. One, on the other hand, looks for anxieties in directions opposite to those followed by the patient. One queries whether simultaneously presented anxieties have not always one focus. Two are cautious and never deliberately aim at uncovering many sources at once. One often concentrates, but has found sometimes that one source can be relieved by opening up another at the same time. One had no definite opinion.

3. RELATION OF AMOUNT OF ANXIETY TO NUMBER OF SOURCES (S.Q. D(11)). *Does the stirring up of anxiety from various sources simultaneously increase or diminish the total amount of apprehension (resistance)?*

The same small majority believe multiple sources increase the total apprehension. Two said they diminish it. One thinks the result varies with the patient and another that, although simultaneous excitation may temporarily increase the total tension, it may be the only way ultimately to diminish it. One stressed the point that it is the quantity of anxiety released rather than the unity or variety of its sources that is important.

4. ORDER OF INTERPRETATION (S.Q. D(12)). *Do you as a rule interpret id impulse first or super-ego reactions, sadistic or masochistic impulses, aggressive or sexual impulses?*

A large majority have no general rule, but vary their practice in accordance with the case. Of these, one said " perhaps aggression before sex " and another " constructive before destructive." Other replies were more definite, but disagreed with each other. Thus one said, " Super-ego before id, aggression before sex " ; and another, " Id before super ego, sadistic before masochistic, aggression before sex." The balance was fairly even between those who interpret super-ego before id, and those who interpret sex before aggression. Some replied " whichever is urgent," and others argue for simultaneous interpretation of love and hate. Several underline the necessity of relating super-ego reactions to id impulses in interpreting. One advises interpretation of whichever factor is most hidden from the patient.

Apart from some general agreements of a not very impressive nature, the answers here showed the existence of great uncertainty as to the best policy of handling anxiety in analysis. Allowance should be made for the fact that the questions were too vague, although they were actually complied from a list of questions put forward by candidates in training at one time or other. Most of these individual questions were

concerned with the ventilation of anxiety, in particular, with the significance of the *spread* of anxiety. Some felt that there were dangers in localising anxiety, others that it is best localised in some particular focal conflict. Incidentally, both policies are followed spontaneously by psycho-neurotics. Hysterics usually localise and obsessional cases displace or distribute. This indicates that the best answer is : first discover the spontaneous mechanisms unconsciously exploited by the patient and be guided by this assessment in attempting to alleviate anxiety. A reversal of the predominating mechanism will certainly evoke anxiety, but the problem is how to alleviate it at the same time.

The question whether the " repressed " or the " repressing " factors should be interpreted first is admittedly old-fashioned, but was intended to bring out views as to the quickest method of resolving conflict and anxiety. There is no doubt that originally more stress was laid on the significance of the repressed. The question on id, ego, and super-ego presented the same issue in more modern terms. But it also functioned as a " test " question to see whether contributors were consistent or whether they might give a different answer to the same question expressed in other terms. The result of this control experiment is interesting. There was less agreement than in the answers to the " repressed-repressing " questions.

It is interesting to contrast the general tendencies brought out by replies to a Questionnaire where the object is to ascertain majority feeling with opinions on the same subject given in individual contributions where the question of majority feeling does not arise. In the case of the " reassurance " issue, a number of

contributions were made by individuals in papers given independently of the Questionnaire. These showed more sharp opposition of views than emerged in Questionnaire discussion. But they also proved that if any individual sticks vigorously enough to a challenging position, he can induce changes of attitude amongst members of the society. The following abstracts illustrate these observations :

On January 18, 1933, Melitta Schmideberg read a short contribution entitled " Some Notes on the Technique of Early Psycho-analysis " (published since in her paper on reassurance, pp. 312–13), in which she discussed, among other things, the advisability of using reassurance at the beginning of the analysis of young children showing anxiety and in acute conditions. The discussion was rather critical of these views, Mrs. Klein and Miss Searl stressing that reassurance is unnecessary if proper interpretations are given in good time and that reassurance is often a handicap to later analysis. On February 15, 1933, Miss Sharpe gave a short communication on " An Alcoholic Phase," mentioning that when she, deviating from her usual way of treating the patient, offered him a cigarette during an attack of anxiety, the patient suddenly exclaimed that now he realised for the first time that the analyst's interpretations of being felt to be a bad mother (which he never believed) were correct.

On June 13, 1933, James Strachey touched on the relation of interpretation to reassurance in his paper on " The Nature of the Therapeutic Action of Psycho-analysis." * His first reference to the subject was indirect. Speaking of the action of " active " therapy, he remarks : " It may be unwise for the analyst to act really in such a way as to encourage the patient to project his ' good ' introjected object on to him. For the patient will then tend to regard him as a good object in an archaic sense and will incorporate him with his archaic ' good '

* *International Journal of Psycho-analysis*, 1934, vol. XV, pp. 127–159.

imagos and will use him as a protection against his 'bad' ones. In that way his infantile positive impulses as well as his negative ones may escape analysis. It will perhaps be argued that, with the best will in the world, the analyst, however careful he may be, will be unable to prevent the patient from projecting these varies imagos on him. This is, of course, indisputable, and, indeed, the whole effectiveness of analysis depends on it being so. . . . It is a paradoxical fact that the best way of ensuring that his ego shall be able to distinguish between phantasy and reality is to withhold reality from him as much as possible. But it is true. His ego is so weak . . . that he can only cope with reality if it is administered in minimal doses. And these doses are in fact what the analyst gives him, in the form of interpretations."

He then considers the relation of interpretation to reassurance directly he remarks : " Both procedures may, it would appear, be useful or even essential in certain circumstances and inadvisable or even dangerous in others." He points out that in the first phase of an interpretation the analyst " induces the ego to allow a quantity of id-energy into consciousness " and so is courting an outbreak of anxiety in a personality with a harsh super-ego. In this connection " reassurance may be regarded as behaviour on the part of the analyst calculated to make the patient regard him as a 'good' phantasy object rather than a real one." He doubts the expediency of this, " though it seems to be generally felt that the procedure may sometimes be a great value, especially in psychotic cases," and believes that it does not favour the prospect of making a " mutative " interpretation. " Since it is of the essence of that phase that in it the patient should make a distinction between his phantasy object and the real one. . . . Thus, whatever tactical importance reassurance may possess, it cannot, I think, claim to be regarded as an ultimate operative factor in psycho-analytic therapy." Later, apropos of the effect of " active therapy " and of " forced phantasy," he remarks : " situations are fairly constantly arising in the course of an analysis in which the patient becomes conscious of small quantities of id-energy without any direct provocation on the part of the analyst.

An anxiety situation might then develop, if it were not that the analyst by his behaviour, or, one might say, absence of behaviour, enables his patient to mobilise his sense of reality and make the necessary distinction between an archaic object and a real one." This, Strachey says, is the equivalent of bringing about the second phase of an interpretation. He concludes: "It is difficult to estimate what proportion of the therapeutic changes which occur during analysis may not be due to *implicit* mutative interpretations of this kind. Incidentally, this type of situation seems sometimes to be regarded, incorrectly as I think, as an example of reassurance."

It is interesting to contrast this last view with the assessment of the same situation put forward by Miss Sheehan-Dare in a short paper, "On making Contact with the Child Patient."* Miss Sheehan Dare put forward views on anxiety and its interpretation which indicate clearly what, in her view, is the advantage of interpretation over reassurance. Taking the case of children who suffer from excessive anxiety over the existence of sadistic transference phantasies, she points out that the anxiety is relieved not just by the fact that the analyst remains alive, thereby reassuring the child that its destructive phantasies have not taken effect. It is alleviated because, through the acts of interpretation, the analyst makes clear that she *knows* what is in the child's mind and yet does not react with anger or in a revengeful way. This implies that in practice the only reassurance factor she allows for in making contact with the child (a situation which, of all possible analytic situations, calls in the view of others most for reassurance) is the *existence* of the analyst.

The existence of so many differences of opinion naturally raises the question: which is the more representative set of answers: also whether discussion of dynamic situations in terms of ego topography is not a source of confusion. However that may be, the

* Sheehan-Dare, *International Journal of Psycho-analysis*, 1934, vol. XV, pp. 435–9.

answers to the questions on reassurance were more definite than those given in individual papers.

5. DEFINITION OF REASSURANCE (S.Q. B(1)). *Since the use of terms such as "deep interpretation" and "reassurance" may introduce some artificial distinctions, what do you understand by "reassurance"?*

A majority defined reassurance as mitigation of anxiety by means other than interpretation, e.g. by suggestion or by reference to reality. Two defined it as the adoption by the analyst of a kindly super-ego (consoling parent) rôle. One regarded it as a deliberate attempt to reinforce the patient's pre-analytic methods of defence against anxiety. Another regarded it as a part of interpretation, inasmuch as it helped the patient to say difficult things. Another said that any means which relieves anxiety is reassurance. Other definitions were: "anything which shows the dreaded phantasies are not real" and "proof that the analyst is good."

6. USE OF REASSURANCE (S.Q. B(2)). *If you accept the term as distinct from interpretation, do you regard it as an essential part of analysis or is it only an emergency measure?*

A majority considered reassurance an emergency measure, two as "rare." Most thought, however, that some degree of reassurance is inevitable at some stage or other in every analysis. Several stated that ideally it should be unnecessary, i.e. the need for it should be obviated by interpretation. One finds interpretation more useful in a crisis. Another uses reassurance sparingly, as a means of dosing anxiety, especially with psychotics. Two regard reassurance as an integral part of analysis.

On February 7, 1934, Melitta Schmideberg presented her views on this subject to the Society.* The paper contained a

* Melitta Schmideberg, "Reassurance as a Means of Analytic Technique," *International Journal of Psycho-analysis*, 1935, vol. XVI, pp. 307–324.

wealth of clinical illustrations of the types of reassurance employed by the author in adult and child analyses which cannot be adequately summarised here. Some of her " key " conclusions were as follows : " The fact that pseudo-analysts use reassurance instead of interpretation or use it in a wrong way should not prevent one using it correctly, that is, combined with interpretation " (p. 322). " The value of reassurance in analysis is similar to that of narcosis in surgery ; it makes the operation less painful to the patient and allows easier working for the physician " (p. 308). Rightly used, i.e. combined with interpretation, it has a number of important functions. " Reassurance may be regarded as a method of dosing anxiety. . . . If the patient is unable to deal with his anxiety owing to lack of satisfactory defences the analyst should temporarily lend them to him " (p. 307). Further, " at times, only the *reassurance makes it possible* for the patient *to accept the interpretation* " (p. 308). " Reassurance has dynamic effect in so far as it helps anxiety to become conscious . . . it is largely due to the normal friendly attitude of the analyst that anxiety becomes conscious, even if it cannot always be related to a special situation " (p. 309). " In my view the analyst's own attitude, mainly in regard to the following points, is an essential precondition for the working of interpretation. How far is the patient a real person to the analyst, of whom he is not frightened and to whom he has a fundamentally good attitude ; how far is the analyst not afraid of the unconscious phantasies and how far does he believe analysis to be something curative and not something harmful. . . . My reassurance will be effective only if the patient feels it is not mechanically done, but is spontaneous and springs from personal interest in him. . . . The more the analyst is regarded as a real person, the stronger the love and hate emotions and the guilt and anxiety reactions will become. Thus the weak solution of the transference emotions and reactions can be intensified through the reality behaviour of the analyst. On the other hand, the more the patient feels that the analyst is a real and helpful person, the more easily he can stand his anxiety " (pp. 309–10). " Above all, the reassurance should serve as a reassurance for the

patient and not for the analyst's own needs. . . . Reassurance gets most of its value through the transference situation. A test for the right type of reassurance, in my view, is if it diminishes the tendency to acting out. . . . Reassurance should have only a temporary aim at easing the situation at the moment and not try to influence the patient's character or symptom. Any alteration in the latter should be brought about by interpretation. It is also important that the attitude of the analyst giving reassurance should not be in contrast to his attitude when interpreting. I believe that the art of giving the right sort of reassurance at the right moment is not easier than giving the right interpretation" (pp. 316–7). "The possibility that reassurance might increase the repression seems to me the most serious argument against its use." But ". . . probably it does not matter even if some anxieties or phantasies are temporarily repressed, so long as by further interpretation one succeeds in analysing them" (p. 322).

In the ensuing discussion, the paper obtained in the main a reception which differed from previous discussions on the same subject in that it might be described as friendly with reservations. Thus Dr. Glover, while commending the paper from the point of view of the Questionnaire, pointed out that while reassurance may increase belief in a good object (proof of this has to be sought in the subsequent course of analysis), it does nothing to reduce the belief in bad objects. Dr. Eder said that he tolerated all types of behaviour in his patients and was not afraid at times to give advice or do something real for the patient, such as removing a foreign body from the eye. He was, however, convinced that reassurance was quite ineffective in a phase of negative transference. Miss Sharpe gave illustrations of useful reassurance and emphasised the importance of unanxious adaptation of the analyst's technique to the varied needs of the individual patient. Dr. Herford recalled Abraham's stress on the need for keeping the analytic situation free and relaxed. Mrs. Klein agreed that the right type of reassurance is as important as the right type of interpretation and emphasised the importance of the analyst's own attitude toward the patient. Over-anxiety to cure is detri-

mental. Dr. Scott mentioned the useful reassurance given by holding the arm of an unmanageably aggressive patient, particularly of giving her warning in advance that he would restrain her if she attacked him. Dr. Heimann said that in a negative transference phase reassurance may increase anxiety about aggression. Miss Searl took the view that failure to interpret brings about the need for reassurance and that the only effective reassurance is given by correct interpretation. She had sometimes reassured and regretted it. There should be no need to prove to the patient that one is not afraid to break any rules of analysis. Dr. Payne stressed the value of the indirect reassurance given by the analyst's detached attitude and freedom from anxiety. The consensus of opinion seemed to be that the value of reassurance varied with different patients and different analytical situations, and that while it might be a useful adjunct to interpretation, it could never be in any sense a substitute for it.

It will be seen that although a definite consensus of opinion exists in the matter of reassurance (with or without interpretation), the question of anxiety and how to deal with it is by no means a settled one. This is only natural. If we knew all about the various forms of anxiety and the appropriate therapeutic procedures the greater part of most analysts' difficulties would disappear. A good deal of the confusion and uncertainty existing on this subject is due to the fact that we are really only beginning to understand the complex nature of anxiety. But there is another source of confusion. As in the case of " deep " interpretation, where the first necessity proved to be an exact definition of the term, so with anxiety it was obvious that the term was used in different ways by different analysts. For instance, it was not at all clear whether the conflicts giving rise to difficulty were thought to have set up

true anxiety reactions or whether the tension was ascribed to guilt. Like other branches of medicine, psycho-analysis is susceptible to the influence of " clinical slogans." There are certain fashions in these attitudes to clinical problems. In the early days difficult cases were accused of " adhesive libido " or " narcissism." Subsequently " sadism " and " guilt " were regarded as the main obstacles; and, more recently still, " bad internal objects " or " psychotic reactions." The one term which has remained more or less constantly in use is " anxiety." Hence there is an increasing lack of discrimination in the employment of the word. Strictly speaking, it should never be used without precise qualification. Does the patient show or experience manifest signs of unmodified anxiety either in its physical or in its mental forms? Or is there indirect evidence that consciousness is threatened with an impending eruption of anxiety, i.e. a fresh charge or an increase of a previously constant charge. These two situations differ in principle not only from anxiety-readiness but also from situations where it is *postulated* that the patient has so-called " deep " anxieties. Presumably " deep " anxieties imply the existence of strongly cathected unconscious situations, any activation of which (whether due to analysis or not) threatens to set the " anxiety signal " system in operation. Presumably fluctuations in these " deep " cathexes can be *inferred* from behaviouristic and analytic evidence. It is not at all clear which of these types of situation were regarded as " anxiety " when the replies to the Questionnaire were given. Moreover, there was a tendency to lump together all manifestations of *mental tension*, direct or

indirect, and to designate them " anxiety " irrespective of their real nature. Although, as Freud suggested, guilt is no doubt a modified form of anxiety, it is scarcely permissible to confuse the clinical manifestations of guilt-tension and anxiety-tension. It is true that overlappings and fusions of guilt and anxiety occur. Anxiety also fuses readily with some depressive effects. But the negative signs of tension, the inhibitions, silences, and reductions of mental activity, which constitute a large part of analytic resistance are by no means due exclusively to anxiety. Quite apart from immediate inhibitions there are many reaction defences of the obsessional type which are built into the character and function as defences against emotional states. *Increased activity* of such character reactions, however, may on occasion be a sign of increased deep anxiety.

In any case, even if we could distinguish with greater accuracy between the clinical manifestations of anxiety, it is unlikely that we could arrive at a flat ruling as to their analytic handling until we could establish their different sources of origin. In the old days this was a comparatively simple matter, since the working assumption was made that most unconscious anxieties belonged to the castration group. Nowadays one hears of " body " anxieties, " introjection " anxieties, anxieties about " internal objects," etc., and although these differentiations are steps in the right direction, they are subject to considerable abuse. In the first place we may have a bias in favour of one type of anxiety and regard all anxieties as belonging to this group. And in the second place we are inclined to be misled into thinking that *acute* anxiety is a proof of *archaic* anxiety. This is not the case. Any psychiatrist will

distinguish between hysterical anxieties, depressive anxieties, and schizophrenic anxieties. Acute states verging on panic are more often than not hysterical. Hence analysts whose practice has been mainly concerned with characterological difficulties are likely to be misled when more volatile psychoneurotic cases develop an acute phase. This is a frequent source of diagnostic error and has led to the description of spurious " psychotic " episodes. Unfortunately not all medical analysts have had an adequate psychiatric training and very few lay analysts have had any psychiatric experience at all. But no doubt these sources of error will be eliminated in course of time and we may look forward to an accurate differentiation of anxieties in terms of their associated content. When this stage is reached it seems likely that the policies necessary for coping with anxiety outbreaks will be correspondingly varied.

In any case, no review of this subject can be considered complete which does not take into account the factors of counter-transference. These may well harden opinion either for or against policies of reassurance. Thus, for example, the tendency to use external illustrations may spring from the analyst's own feeling of uncertainty regarding the validity of the particular interpretation. This may be detected by the patient and thereby lose reassurance value, possibly increase anxiety. On the other hand, a rigid policy of withholding reassurance may arise from the analyst's negative counter-transferences. Rigidity in practice on all matters involving the discomfort of the patient should be carefully scrutinised from this point of view. The whole subject certainly requires careful review from time to time.

CHAPTER IV

INTERPRETATION—III

Various Problems in Interpretation

1. USE OF TECHNICAL TERMS : EXPLANATION OF MECHANISMS (Q. 3. (1)). *What practice do you favour,* e.g. *do you talk about introjected objects or organs or do you use super-ego nomenclature?*

This question proved to be one of the half-dozen on which the answers showed almost complete agreement. Official objection to the use of technical terms was nearly unanimous. The great majority invariably use everyday terminology familiar to the patient except in very special circumstances, e.g. to avoid " snubbing " an informed patient, or as a short-cut with such a patient. Only three definitely use super-ego terminology or refer to introjected objects. They justify this on the ground that it is a useful technical jargon which may promote clarity in explanation, on a par with the use of technical terms in other fields.

In the original Questionnaire this point was raised as a minor problem in interpretation. Strictly speaking, it might have been included in the question relating to Form (Q.I. 1, p. 16). " Do you prefer . . . trying to convince by tracing the development of a theme ? " ; because the use of technical terms is one way of appealing to the patient's intelligence or of reinforcing his preconscious conviction through an emphasis on theory. As it happened, the fact that it was put as a

separate question provided another spontaneous test of consistency. There was much more general and definite objection to the use of technical terms than to " trying to convince " the patient. One might conclude from this either that the views expressed in favour of producing intellectual conviction are weakly held or that using technical terms arouses special guilt. The inconsistency, however, is not absolute, since one may try to convince without necessarily using a single technical term.

Another aspect of the subject was unfortunately omitted from the Questionnaire. An additional query should have been included : in giving interpretations of the patient's psycho-sexual phantasies or activities do you use neutral (scientific) terms in place of more usual (frequently obscene) terms ? A question of this sort would have raised a number of matters of principle, e.g. unobtrusive ways of reassuring or stimulating the patient, the maintenance of transference neutrality, the assumption or inculcation of implicit moral (super-ego) attitudes, the limits of expression of counter-transference. Although apparently a minor point this should be worked out in some future enquiry. In the meantime we may assume that the most valid objection to the use of technical terms arises when they have no " psychological meaning " for the patient.

2. EXPLANATION OF SYMBOLS (S.Q. D(1)). *Do you explain the nature of symbols in dream interpretation ?*

There was apparently some doubt as to whether the word " nature " was intended in the theoretical or " content " sense. Only two ever give any theoretical explanation. As regards " content " one-third interpret symbols freely, one

third very much less freely, i.e. " sometimes," " seldom," " sparingly," and a few replied " not at all," but perhaps these refer to explanation of the theory of symbols.

Actually this question referred to the observation frequently made by candidates that in associations to dream material some classical symbols unfold themselves, as it were, and show very clearly the unconscious processes leading to their formation. These, they find, can be interpreted with more certainty than other symbols which give no self-revealing associative connections whatsoever and must be interpreted (if at all) in a more arbitrary manner, i.e. in accordance with a general consensus of opinion as to their meaning. The latter type provoke more doubt in the minds of both analyst and patient. Hence there is a temptation to follow up and demonstrate to the patient the associative chain illuminating the former type.

Apart from this, it is interesting to find there is a variation in the exploitation of symbols in dreams. Obviously some analysts must prefer estimating what current events or situations lead to the activation of certain latent content, and others seize on symbols as a quick way of assessing unconscious phantasies apart from their relation to current factors.

3. RECONSTRUCTION AND RECOVERY OF MEMORIES (S.Q. D(2)). *Do you think " reconstruction " helps or hinders the recovery of memories?*

Answers here showed considerable variation and indecision. A third refused to generalise. Half gave definitely positive, if tentative replies to the effect that reconstruction is an aid to the recovery of memories. Of these one or two think reconstruction more likely to be helpful in later stages than in early stages

of analysis. One regards it as helpful in proportion to the analyst's certainty about specific details. Two think reconstruction hinders the revival of memories. One considers that careful analysis should enable the patient to reconstruct for himself, and another that only transference analysis really helps.

This question was worded as above because it had been raised in this form on a number of occasions. Obviously many candidates tend to assess the progress of their analyses in terms of " recovering memories " and are uncertain of the function of reconstruction, e.g. whether it is a substitute for recovery or a stimulus to recovery. In these particular respects the answers are not very helpful. But the question has another side. It touches on the problem of long or short interpretations (*q.v.*, pp. 17–20). If reconstruction is an aid to or substitute for recovering memories, then clearly long interpretations are sometimes called for, since it would appear difficult to reconstruct effectively in a few sentences. Considerations of this kind suggest that the rules regarding long and short interpretations should be determined on clinical grounds. For example, since interference with memory is a characteristic of hysteria and *recovery* of memories more frequent in hysterical conditions than in any other, two conclusions follow : first, that reconstruction should be avoided when possible in hysteria and, second, that as a rule interpretations in hysteria should be short.

4. VALUE OF CHILDHOOD MEMORIES (S.Q. D(3)).
Do you attach special value to childhood memories ?

A large majority attach great importance to the recovery of childhood memories. (One analyst asks in this connection

" What is psycho-analysis ? ") Of these a number qualify their answers, e.g. they do not value memories as memories but in relation to transference analysis, or, they attach value to " traumatic memories accompanied by affect." One or two value memories in so far as they can be linked with phantasies, i.e. they regard phantasy as of primary importance. One thinks the value of a memory varies with the depth of its repression. Revival of memories is also regarded as a sign of progress in analysis.

5. CLINICAL TYPES AND RECOVERY OF MEMORIES (S.Q. D(4)). *In what clinical type do you meet with the maximum of recovered memories ?*

Several failed to answer this question at all. There was also some confusion between memories recovered during analysis, and total of memories accessible. Of those who did reply, a small majority found hysterics recovered most memories. The remainder exhibited many different opinions, e.g. some suggested that the maximum recoveries occur in delusional cases, traumatic cases, psychotic cases, paranoid, cyclothymic, etc. One only replied that the maximum number occurred in cases of obsessional neurosis while another said that obsessional cases were poor in recovered memories.

A discussion on May 2, 1934, on " The relative importance of reviving memories and reconstruction " did not add a very great deal to the answers received. The resistance value of reconstruction and its possible use as flight from transference and affect were mentioned. There appeared to be a feeling that memory-revival is no longer of primary importance or at any rate not so important as it was considered when repression was the chief defence-mechanism recognised. It was held that it is as important to follow out the interplay of projection and introjection in development, as it is to revive early memories. The importance of body memories, and their liability to projection, was also mentioned. The importance of relating affect to memory, of linking up isolated memories, and of tracing the wealth of detail scattered through numerous phantasies to central events in early life was also stressed.

An interesting possibility was mentioned that patients may discover a predilection on the part of their analysts for recovered memories and seek to gratify this.

Behind all these doubts and uncertainties there exists an important clash between two points of view. One is strongly supported by the traditions of psycho-analysis and the fact that analysis was first and most freely applied to the psycho-neuroses. The tradition is that in an ideal analysis the patient should have revived most of his repressed memories (alternatively, reconstructed most of his infantile life), and have a good emotional understanding of the main features of his psychological development, particularly of those factors leading to the original infantile neurosis. The opposing view is that although recovery of repressed memories and reconstruction of infantile life are important, analysts may attach too much importance to the relation between the child and his actual environment. They would argue that although all ideations are based on psychic experiences, still there are certain ideational forms (unconscious phantasies) which are specially influenced by endopsychic factors (instinct drives and unconscious mechanisms), and are perhaps more prolific sources of conflict than the actual relations of the infantile ego to the external objects of its impulses. Naturally this school would disclaim any intention of depreciating the significance of environmental influences whether traumatic or benign, episodic or of a more continuous nature. On the other hand, those in favour of memory reconstruction are just as emphatic in asserting that they do not depreciate the significance of more purely endopsychic factors and

are just as interested in unconscious phantasy systems. If the disclaimers of both groups are sound then obviously there is nothing to argue about and it would follow that any existing disputes are due to extrinsic (i.e. subjective) factors. These subjective factors might be of a general nature, e.g. a need to dispute; or they might be influenced by preconceived and rather moralistic ideas. It is perfectly clear that if emphasis is placed on environmental factors, particularly of a traumatic order, the implication is that with better care on the part of the parents or "child-minders" these "pathogenic" stimuli might have been avoided. There is here a more or less direct aspersion on the parents. On the other hand, if emphasis is placed on the pathogenic influence of purely endopsychic factors, the implication is that the parents need not necessarily have been "to blame." The onus in that case would be on the child's own psyche. And although most analysts are nowadays ready to arrive at a gentlemen's agreement, this was not the case in 1933. A change in attitude on this point has gradually come about which takes the form of saying that endopsychic and environmental factors inevitably dovetail. But the temptation still exists to be influenced in the valuation of analytic material by theories as to the ideal relations that should exist between children and their parents.*

Another interesting possibility is that difficulty in assessing the comparative significance of endopsychic

* At a Discussion on June 29, 1938, James Strachey made a remark to the effect that when analysts are hostile to their parents they may incline to favour environmental factors, and when they feel that children are a nuisance they may tend to blame endopsychic factors.

and environmental factors arises from a subjective preference on the part of the analyst for one particular type of unconscious mechanism. It is likely that those whose own introjection mechanisms are unusually active and who therefore attach more importance to the introjection defences of their patients will tend to depreciate and frequently to overlook environmental factors, while those who are more concerned with projection in themselves and in their patients will correspondingly underemphasise the endopsychic factors. In the meantime, it seems desirable to assume that, despite the assurances and disclaimers of both groups, there is a genuine scientific issue at stake, viz. the nature of the interrelations between the psyche and its environment. At least, it is plausible that there is a variety of such interrelations and that in different psycho-pathological states these variations come to a characteristic expression. In any case it is the objective facts about interaction that we have to arrive at and to assess. Pooling and comparison of individual findings is the method most likely to correct " personal " distortion and prevent extravagances arising from subjective sources.

6. EXCESSIVE LOQUACITY (S.Q. D(5)). *What is your most successful line of interpretation dealing with excessive loquacity?*

The answers to this question were varied. Two evidently appreciated the multiplicity of conditions under which loquacity may occur and replied " Cannot answer in a word " and " Cannot generalise." A third said the line taken varied with the patient's defences at a given time. A fourth tries to stem the flow by picking out words or " slips." The remainder tended to fall into two groups. One group concentrates on

interpreting the libidinal meaning or function of speech. Examples of these are " meaning," " oral erotism," " Anal,' " Urethral-anal." The other group go for the defensive function of the activity : thus " Look for avoided subject," " Why afraid to pause ? " Three rely expressly on transference interpretation but do not specify whether they stress the libidinal or the defensive aspect.

These answers bring out the essential weakness of the Questionnaire method as applied to analysis. A large number of " difficulties " had to be omitted from the list (*vide* following questions) because they arose out of some special situation in a particular analysis. These were suitable for seminar discussion only. As soon as a clinical difficulty is sufficiently general in nature to be put within the compass of a short question, replies tend to be equally general and consequently not very helpful.

Although the persistent silences of some patients are a more common source of trouble, some analysts have evidently found themselves perplexed by the companion problem of persistent loquacity. Actually, from the resistance point of view, loquacity is a more stubborn defence than taciturnity. Perhaps the best answer is that it depends on the diagnosis. The loquacity of obsessional types is usually accompanied by an ambivalent attitude to interruption. Such patients pause to throw cold water on the analyst's interpretation and then resume their discourse. Here fairly constant interpretation of the transference ambivalence would seem to be indicated. Paranoid types are often loquacious and react still more sharply to " interference." The most difficult types belong to the euphoric group. Generally speaking direct inter-

pretation of loquacity has little effect on psychotic types. Interpretation in such cases should be regulated by a sense of the " urgency " of deeper conflict, particularly conflict over pregenital phantasies. The immediate transference significance of these phantasies should be stressed.

7. OBSTRUCTIVE COMMON SENSE (S.Q. D(6)). *What is your most successful line of interpretation dealing with obstructive common sense ?*

The answers here were also very varied but a fairly strong tendency appeared to interpret " common-sense " as defence against " phantasy," especially transference phantasy. The factors most frequently referred to were those of anxiety and defensive aggression and interpretation along these lines was suggested. Some analysts use a device other than interpretation, e.g. one seizes on a weak spot in the patient's argument, another queries something that the patient assumes to be obvious : " Obvious ! Why ? " One concedes any reality in the patient's statement but insists there is " more behind." The most philosophic reply was " Let them talk, they get over it." One analyst regards this as an intractable form of defence, only amenable to a strong transference drive to overcome it.

8. ENCOURAGEMENT TO TALK (S.Q. D(7)). *What is your method of dealing with patients who need constant encouragement to talk ?*

These answers were rather indefinite, but tended to fall into two groups, similar to those observed in relation to the problem of loquacity (S.Q. D.5), one group emphasizing the libidinal values, the other, the defensive anxiety connected with this need. Some speak more themselves with such patients. One is ready to accept alternative material, e.g. a letter. This type of resistance appears specially liable to rouse anxiety in the analyst, as evidenced by resort to reassurance, desire to overcome it quickly, etc.

This question was unfortunately worded. Intended to extract information on the best way of dealing with patients addicted to silence, the use of the phrase " need constant encouragement " seemed to imply that reassurance was the correct procedure. Actually a number of clinical varieties of analytic speech-inhibition exist, e.g. cases, either extremely hysterical or border-line psychotic, who begin analysis with a more or less persistent silence ; or cases who, between little sporadic bursts of communicativeness, are apparently stricken with a painful or anxious incapacity to get anything out. As in the case of loquacity perhaps the best procedure is to diagnose the particular clinical variety of silence and use the diagnosis as a guide to the timing and nature of interpretations.* But this does not solve the problem of reassurance. Incidentally, if reassurance in order to promote talking is the correct procedure, there seems to be no reason why we should not inform loquacious patients that they need not be alarmed if nothing at all comes to their minds. It is quite certain that loquacious types become restless and tense on the rare occasions when they fall into a silence.

9. SPONTANEOUS OFFER OF INTERPRETATIONS (S.Q. D(8)). *What is your most successful line of interpretation dealing with the spontaneous offer of infantile interpretations by the patient ?*

The answers to this question showed more agreement. The majority treat it as a transference manifestation, a positive, negative or ambivalent manifestation, or an attempt to lead the analyst away from an " anxious " transference situation.

* The inability to speak displayed by persecutory types has been commented on by Melitta Schmideberg.

This general consensus of opinion suggests the necessity for correction in terms of counter-transference. There is, of course, no doubt that spontaneous interpretations on the part of the patient are most often transference manifestations, " displacing the analyst," " stepping into father's (or mother's) shoes," etc. But it can have genuine validity. Obsessional types frequently give very accurate part-interpretations of their own material, remaining, nevertheless, devoid of emotional appreciation of their significance. Hysterics (particularly conversion cases) and depressives are usually poor self-interpreters, although the latter frequently offer accurate guilt interpretations in order to cover their deeper paranoid attitudes. So-called normal cases are also poor interpreters but often conceal the fact by assiduous reading of analytical literature. Schizoids are often very good indeed and it is well to pay attention to their interpretations.

In all such cases a counter-transference reaction should be looked for. However tolerant the analyst may be, the usurpation of his most godlike privilege, viz. that of telling other people the " true truth " about themselves is calculated to arouse counter-transference reactions. Yet no analyst need be ashamed to be instructed by his patient. Few analysts can hope to be expert in the handling of every type of case. If their own mechanisms correspond with those of a particular group they may be very good with that group (despite an obvious risk of occasional complete misinterpretation due precisely to that correspondence of mechanisms). Similarly with dream interpretation : each analyst has a flair for special types of dream, but on many occasions

his patient's flair should be respected. A good schizoid case will often teach an analyst more about symbolism than can be learned elsewhere. It must often happen that the patient has really better inside knowledge than the analyst, though, of course, he is seldom able to use it for anything but resisting the analysis skilfully.

Counter-transference may also be responsible for a certain amount of suspicion on the analyst's part when he finds his patient co-operative. This, of course, would reflect the analyst's own doubt of his own interpretations.

It is to be noted that nearly two-thirds of the contributors failed to reply to questions S.Q. D5 to 8. One remarks, " all these are crystallised ego-defences and only give way gradually."

10. WORKING-THROUGH (S.Q. D(9)). *Do you allow time for " working through," i.e refrain from additional (new) interpretations for a time after uncovering highly charged emotional situations ?*

A majority replied in the affirmative : " Yes " or " as a rule." Some mention that further interpretation may be necessary to allay anxiety. One allows time for working through, but does not " allow the emotional situation to be exploited " indefinitely. A few vary their practice according to the patient. Others do not allow time as a matter of routine.

This is yet another instance of the need for a preliminary agreement as to definition. For it appears from the replies that there is no general acceptance of Freud's own view of the nature of working through : that it is a spontaneous psychic process which is perhaps the only effective means of countering " the resistance of the id." Incidentally, this view would

justify a good deal of patience with the process of self-interpretation.

Probably this difference of opinion is due to differences in character between two groups of analysts. Those who are vigorous and indefatigable do not take very kindly to the idea of the patient's " working through." They suspect that the " working through " notion is a cloak for lack of understanding, as is no doubt frequently the case. Other, more expectant, types are inclined to respect the self-curing tendencies of the mind and regard analysis as a process of redressing balances. Their analytic interpretations are intended to free the patient's ego thoroughly enough to permit it to look after its own internal concerns. This view involves certain quantitative considerations. It can frequently be demonstrated that a rapid alteration in the symptom picture is due to a reduction in *marginal* anxieties, i.e. the interpretations release *only* that amount of anxiety which permits more effective action of repression. Despite symptomatic improvement, this outcome is not entirely satisfactory. A more ample margin of stability is desirable. But it does bring out the fact that during the course of many analyses the ego uses therapeutic methods of its own manufacture. Nevertheless, it is obvious that quantitative factors of this sort cannot be ignored. Even with the most comprehensive and thorough interpretation no standard length of analysis can be established. Patients having practically identical symptoms vary in the rapidity of their response to identical interpretations. These considerations suggest that the concept of " working through " is not simply a defensive concept, an apology on the part of the analyst for the

length of his analyses. In other words, some patients cannot be and some patients should not be hurried. We may agree, therefore, that although analysts may on occasion advance " working through " explanations in self-defence, there are (particularly in psychotic types) certain deep transference attitudes (hurt, suspicion, ambivalence, etc.) which require prolonged periods of ventilation. As for permitting " working through " periods after active interpretation, that policy seems to be merely taking a leaf out of the book of spontaneous mental defence. It is easy to observe that after any crisis either in analysis or in ordinary life most individuals tend to " draw in their horns " until the situation has been stabilised. This recuperative process no doubt depends on a system of " working through." Finally, it is possible that analysts dislike the process because it is felt to be an aspersion on the virtue of their interpretations. And no doubt it would be rather mortifying to think that an unusually prolonged analysis was successful, not so much because of the effect of long continued interpretation, but because the patient has gradually succeeded in curing himself. This mortification would not be justified unless the patient had cured himself in despair. For there is no doubt that many patients who do cure themselves in analyses would not have been able to do so unaided.

CHAPTER V

INTERPRETATION—IV

FREE ASSOCIATION, FORCED PHANTASY AND "ACTIVE" DEVICES *

1. FREE ASSOCIATION (S.Q. C(6)). *Do you keep strictly to the free association rule or permit (advise) relaxations of it?*

A majority permit relaxation. Several answers emphasise the impossibility of preventing relaxation. One queries "everything is free association, what are relaxations?" Only a minority keep strictly to the rule whenever it is in their power to enforce it, but admit it has sometimes to be relaxed. A few imply that they work mainly by the rule but relax for definite reasons, e.g. deliberately concentrate attention on some topic either because they feel that it is a source of special difficulty or because they wish to deflect attention from another. Two never advise relaxation, whereas another two both permit and advise it. One does not even feel it necessary to recommend the rule to the patient.

There has evidently been some slight misunderstanding here. Not everything in analysis is free association: suppression of conscious ideas is not free association. The question should have run: Do you permit or, on occasion, encourage the patient (not so much to suppress ideas permanently but) to delay communicating them? It is probably only in psychotic

* See also Part II, "Transference and Routine," p. 91.

cases that the issue is worth very serious consideration. A more important question is whether the analyst unwittingly overlooks the constant omission of affective expression in analysis. The form of the association rule most frequently communicated to patients seems to be : " Say what is in your mind." And this is taken by the patient to mean : " Say what you are thinking." Whereas if the instruction were : " Tell me also all about your *feelings* as you observe them rising into your conscious mind," in a great number of cases the ideational content would follow of necessity. Naturally this would not apply to such cases as have developed specific affective defences, e.g. can dissociate affect from consciousness. Policies about affect require much more attention than they have hitherto received.

2. FORCED PHANTASY. (*The term " forced " (Ferenczi) is not very satisfactory : the interpretation of an incest phantasy is equally " forced."*) *The question is better stated as follows:—*

ABANDONMENT OF RULE TO RECONSTRUCT PHANTASY (Q.4. (2)). *Do you abandon the association rule in certain instances, holding the patient to one thread until you have constructed a phantasy piece-meal ?*

About half abandon the rule in some instances. One-third encourage expansion of phantasies.

These answers are, at any rate, consistent with those given concerning free association (p. 66) : for if the analyst holds the patient to a particular thread he is obviously himself breaking the association rule. In the same way, of course, he breaks the rule if he gives

preference to dreams and keeps asking for associations to dream items until he has run them through. It is evident that there is less timidity in replying to this question, possibly because the phrase " free association " has more traditional sanction, and the word " forced " is suspect.

3. INTERPRETATION OF ISOLATED WORDS AND ACTIONS (Q.4 (1)).

Do you, on the strength of isolated words or actions, without waiting or hoping for confirmation in associations, expound phantasy systems affecting (a) reality relationships (b) transference relationships?

A majority sometimes interpret from details, e.g. when sure that they relate to some immediate anxiety. One-third, on the other hand, stated that they never interpret from details. The remainder vary in habit.

In this question a distinction ought to have been drawn between expounding phantasies on the strength of details (*a*) irrespective of difficulties; (*b*) when difficulties, particularly prolonged silences, occur; (*c*) at the beginning of sessions before much free association has taken place. Naturally, when silences are prolonged there is often little left to interpret save isolated words or movements. On the other hand, even when associations are fluent it is often the case that the most important material is contained in the first few phrases or in some apparently trivial gesture made on entering the room. If the rest of the associations gives no clue to these preliminaries, it would seem admissible, sooner or later, to ignore the bulk of the associations and interpret the preliminary material. Obviously this could be done, if necessary, without

delaying for further associations especially if the material of the day before had led the analyst to anticipate resistances on the following day.

This raises a point which has not been discussed fully enough. The analyst is in the habit of applying dream technique to dream material, in particular undoing the process of condensation by isolating items and applying associative methods to them : but there are doubtless many fragments of conscious phantasy, stereotyped phrases, verbal mannerisms, quotations, and clichés that should be dealt with in the same way, viz. by isolation and special investigation.* Where the underlying material appears to be especially archaic (i.e. is not interwoven with pre-conscious thinking) the application of dream technique, e.g. symbol reading, to conscious " scraps " would seem justifiable. In any case the interpretation of " scrap " material, particularly at the beginning of a session, should usually be in terms of transference.

4. QUESTIONING THE PATIENT (Q.4 (3)). *Do you ask direct questions : (a) about matters of fact,* e.g. *family history ; (b) about matters of phantasy ; (c) about emotional reactions ?*

A majority ask questions freely, others occasionally. Some said they never do so in the early stages of analysis. One who has no objections on principle and occasionally does ask questions, finds the method of too little value to be worth while.

Here again the answers are fairly consistent. If analysts made a fetich of the association rule it would be inconsistent of them to ask questions. Apart from

* *Cf.* the point of view expressed by Ella Sharpe, *Dream Analysis*, 1938, Hogarth Press.

dealing with the habits of the analyst, this and the previous three queries are really intended to find out whether analysts are slavish in obedience to their own general rules. Clearly the answer is that they are not. A further question of a more personal nature would be whether they are easy in their minds or guilty when they relax their accustomed practices. Also, whether they relax most with easy or difficult cases. Apart from this it would seem quite natural to ask patients about matters of fact concerning which one was in doubt. Some patients are put at their ease by being asked questions. And many family situations would be grasped much more quickly by the analyst if he did not hesitate to ask about some detail instead of waiting until the detail emerged in the natural course of association. It is a question of the balance of advantages. He may wait too long or he may drag the detail from its legitimate context. The suppression or omission of minor details is frequently of great psychological significance. It would be well, therefore, to be familiar with the patient's habit in these matters before deciding to use or eschew questioning. Naturally there should be no intention of " history-taking," a procedure that would certainly stultify free association.

" Fishing for affect " is a problem of a different kind (see 1 in this section). One question of importance was unfortunately omitted, viz. " Do you ask the patient about his subjective feelings in the matter of therapeutic progress, i.e. whether he feels better or worse ? (See Termination of Analysis, p. 111.)

Discussion (October 18, 1933). Reference was made to a definition of " forced phantasy " as " definition in the dark." The risks of such interpretation were held to be less in the

case of transference phantasies. Miss Sharpe reported that she found it useful in patients with active reality interests to seize on similes and odd remarks, especially where these appeared likely to conceal some concrete meaning (see footnote, p. 69). Mr. Strachey suggested that stopping to ask questions about some obscurity, especially early in analysis, might lead to missing other important things. Dr. Jones, however, contended that equally important things might lie behind the obscurity. Dr. Schmideberg referred to the anxiety which questioning might rouse in patients, since it is so easily interpreted by them as attack.

5. PLAY TECHNIQUE AND ADULT ANALYSIS

(S.Q. G(2)). *Do you favour adopting any form of play technique in adult analysis* (e.g. *providing paper and pencil, etc.*) *?*

A majority were not unfavourable but only about one-third have found such measures necessary, or made much use of them. Thus one encouraged a silent patient to write and another provided pencil and paper in one case. Yet another would provide anything that would help the patient to express himself. There is certainly no general taboo on the subject, but the device is sparingly used on the whole, and then usually limited to such relatively adult methods as writing, drawing and making diagrams, etc.

The answers to this question were disappointing. In earlier times it was natural that the fundamental rule of free association should be treated as fundamental, hence that there should be a conservative reaction against any attempts to alter it. As has already been noted, the original rule tended in practice to discriminate in favour of verbal expression and thereby to encourage limitation of affective expression. In later times the protracted controversy over " active

(Ferenczi) technique " (see p. 91) was likely to produce an equally conservative reaction when the issue of " play " technique in children came to the fore. Actually the development of child analysis was bound sooner or later to raise the issue of modifying adult technique in some directions. If play technique were reserved for small children only, or if an age limit were fixed beyond which play technique could be regarded as inappropriate or unnecessary, there would be no problem. But since child-analysts use these methods on latency cases and even beyond pubertal phases, the matter cannot be left there. Before child technique was standardised many quite small children were analysed by a method of free observation combined with interpretation of observed speech and conduct in the consulting-room. It was also quite common then to have children of seven to fourteen years of age lying on the couch associating in a (sometimes) surprisingly adult manner. It seems obvious that at certain phases adult and child methods should overlap, so that the question of permitting or encouraging play activities in adult cases cannot be burked.

It is easy to observe that some adults, within the limits of a recumbent position, play with every available object in their pockets, bags, or on side-tables. Others endeavour to widen the range of their activities. Others complain that they frequently could draw better than they describe. Others again complain that the main drawback and source of uncertainty in analysis is is the fact that they are not supposed to play. The issue deserves further consideration under two heads : generally speaking, in what types of adult case should play activities be permitted ; in cases of psychosis

should they be encouraged and, if so, under what conditions and with what limitations.

A cognate problem which was unfortunately omitted from both questionnaires is the amount of relaxation permitted in the matter of associating in a recumbent position. This might have been raised in a number of contexts, e.g. how to deal with anxiety crises, the rôle of reassurance, etc. It is a common observation that many patients, particularly men with repressed " passive " tendencies or of slightly paranoid disposition are reluctant to lie down. Women with excessive masochistic phantasies of coitus are also sensitive on this point. Others of both sexes cannot bear the idea of scrutiny from behind and feel they must face the analyst or stand whilst the latter sits : others again want to walk about or lie on the floor or sit in an opposite corner of the room. These wishes or needs present no problem in play-technique, but they frequently cause difficulties in the more conventional adult analyses. The more these details of practice are considered the more obvious it becomes that many of the more conventional replies to the Questionnaire were returned by analysts whose practice is limited to mild psycho-neuroses or character difficulties occurring in otherwise fairly stable individuals. Analysts who deal with a number of acute psycho-neuroses or genuine " borderline " psychoses have to face many more perplexities as to conduct and are less likely to be satisfied with the more conventional forms of advice.

In this connection it is remarkable that no reference was made in either Questionnaire to the problem of " abreaction " or " catharsis." How far, for example, do analysts permit or encourage the abreaction of

affect when this takes the form of smashing objects, throwing them about, or slinging them at the analyst. Although the modern preoccupation with anxiety manifestations tends to obscure the fact, behaviour of this sort has to be distinguished from anxiety crises which may also give rise to violent demonstrations. Moreover it should be distinguished from the violence accompanying some guilt crises. It is essentially repetitive, and the form of the behaviour is consequently much more stereotyped. It has also a considerable " recovery " value, being in fact frequently a substitute for reviving memories. But since it is not so readily amenable to immediate interpretation, the problem arises whether, when interpretation fails, it should be reduced by reassurance or restrained through super-ego injunctions. Reassurance certainly reduces abreaction, but on the other hand, a certain amount of abreaction reduces the patient's compulsive tendency. Uncertainties in dealing with this problem are frequently due to the analyst's subjective reactions to " scenes." Many analysts dislike having small objects of furniture destroyed and patients are quick to spot this fact which inevitably reminds them of childhood experiences and prohibitions.

CHAPTER VI

TRANSFERENCE AND ROUTINE—I

IN THE PREVIOUS section transference phenomena were dealt with only in so far as they referred to the technique of interpretation. Yet many of the answers raised problems of transference and counter-transference. In the present section, apart from a preliminary question, transference is dealt with in a rather matter-of-fact way. Emphasis has been laid on various difficulties arising out of the routine of analysis. The bearing of these routine matters on transference will in most cases be obvious either from the question or from the answers given. The preliminary question (*vide seq.*) was raised in the Supplementary Questionnaire because of the divergence of views on the technique of interpretation brought to light by the first Questionnaire.

A. TRANSFERENCE AND COUNTER-TRANSFERENCE

1. TRANSFERENCE ANALYSIS (S.Q. E(1)). *Do you analyse the transference situation only if it is so outstanding that it cannot be avoided : or if it is a source of resistance : or do you regard it as the main therapeutic device ?*

The balance of opinion appeared to be definitely in favour of analysing transference " throughout," " constantly," " whenever interpretation is possible," etc. One holds " only transference interpretations effective." A few qualified this by phrases such as " when outstanding," " when the transference

neurosis is established," but it was suggested that pretty continuous transference analysis is the usual practice. A large majority regard transference as the main therapeutic device. A few again qualified their answers, e.g. " one of the main therapeutic devices," but there were no negative replies. One answer referred to the need to analyse " all " transference manifestations, i.e. extra, as well as intra-analytic situations while another emphasised the supreme importance of negative transference analysis.

There is slightly more agreement on this point than might be gathered from the answers to the questions on interpretation. This is perhaps due to the form in which the question is put. If approached directly on the question of transference, analysts are likely to be on the defensive since the analysis of transference is by common consent one of the hall-marks of analytic technique. Allowing for a certain latitude in practice, there seems to be no doubt of the general policy of analysing transferences. How " continuous " that routine is seems open to some question. In any case it would have been advisable to find out exactly what each individual means when he uses the phrase " analysing the transference." As has been pointed out (p. 33) displacement is by no means the only mechanism involved. Yet to judge from many interpretations reported, it seems that for many analysts transference is regarded merely as the displacement of personal reactions from past parental relations to themselves.

2. PERSONALITY OF ANALYST (S.Q. E(2)). *How far do you think the personality of the analyst plays a part in the conduct of analysis?*

More than half the replies agreed that the personality of the analyst plays " a considerable part," " a more important part than is always realised," etc., in the conduct of analysis.

Amongst these some regret this influence, e.g. " unavoidably ; ideally minimum." A small minority take the opposite view, e.g. " Very little part," " less than one might think." One said " not large unless technique faulty." Another " some importance but skill more important." One answer suggested that the personality probably affects the order in which transference situations develop and the sequence of material.

Division of opinion indicates the necessity for much more detailed examination of this issue. A supplementary question might have made the matter more pointed, viz. " Do you think that the analyst's own mental devices (i.e. his own favoured unconscious mechanisms which operate irrespective of the degree of his personal analysis) affects the order of the material analysed ? " Even if analysts do not have a blind spot for their own particular mechanisms they certainly have a more empathetic reaction to the use of similar mechanisms in their patients and, unless this were constantly controlled, there would be some likelihood of selective analysis of material. To judge by views presented during scientific meetings it would appear that where there is less necessity for discretion the same people tend to discuss quite different subjects from a fixed angle of interest. This is easy to detect when individual members have special interest in one particular libidinal phase or one particular stage of development or unconscious mechanism or etiological formula or are biased in favour of a particular set of unconscious phantasies. There is a certain stereotyping of comment which more than any other factor is responsible for the dullness of scientific discussion. Hence selective analysis is one of the obvious though by no means the only way in which a personality factor might

operate (see also Section G, p. 106). It is probable that, particularly in the early stages of analysis, the personality of the analyst has the effect of stimulating or reassuring the patient's anxieties or hostilities. And, of course, Freud attached considerable importance to the effect of the patient's early identification with the analyst, a situation that is no doubt either promoted or hampered by his intuitive assessment of the analyst's actual character. Not enough work has been done on the effects produced by change of analyst. Studies of this sort ought to make clearer what these " personality " factors really amount to.

3. EFFECT ON ANALYST (S.Q. E(8)) *Do you find the practice of analysis acts as a therapeutic procedure for the analyst or not ; does it increase or diminish his own conflicts ?*

Answers here were varied and rather confusing, apparently because there was a tendency to equate diminution of conflict with therapeutic effect. A majority consider the total or dominant effect is therapeutic, provided that the temporary exacerbations of conflict which may be produced in the course of treatment are appreciated, i.e. analysed. It was recognised that the analyst is subject to a process of continuous conflict-stimulation which is probably without parallel. This effect is increased in proportion to the number of cases seen daily. A minority replied in the negative, " not therapeutic," and regard such a notion as " suspect." On the other hand, some point out the wealth of opportunity for sublimation and even for direct satisfaction (e.g. of curiosity) offered by the practice of analysis.*

* Barbara Low writes : " The analyst's successful achievement, for himself and patient alike, can best be described if we turn again to Freud and his picture of the artist. The artist, he tells us (for artist we may here substitute analyst), in contact with the external world (for which we may substitute patient) obtains his material, moulds and

The majority opinion that the dominant effect of analysis upon the analyst is therapeutic should be reassuring to intending practitioners inasmuch as it presumably embodies the results of personal experience. The proviso implying that temporary exacerbations of conflict are unavoidable underlines the importance of rendering analysts capable of adequate self-analysis if they are to surmount such crises unaided.* The minority who " suspect " the idea of therapeutic effect probably have in mind the type of person to whom a career in analysis seems to offer hope of vicarious solution of conflicts or the patients who develop analytic ambitions during treatment which disappear as they get better. But no one suggested that practice might increase the rigidity of the analyst's preferred defence systems. Nor did anyone envisage the possibility that the need to remain in touch with feelings and situations normally repressed might lead to accentuation of mechanisms less obviously detrimental to insight. However this may be, any beneficial effects on the analyst should perhaps be subsumed under the term " counter-transference therapy." Presumably such therapy would occur in different individuals for different reasons. In any case the fact that the practice of analysis may be a legitimate sublimation should not be ignored. What these replies do reveal is the present lack of adequate and detailed information on the

illuminates it by fusion with his own unconscious, and presents it again, thus re-shaped, in forms acceptable to reality demands and to the unconscious of the world (the patient). Through such revelations he obtains a means of release, both for his fellow-men and for himself (*I.J.P-A.*, 1935, vol. XVI, p. 8)

* Freud's last recommendation was that analysts should submit themselves to further analysis at five-year intervals as a routine. *I.J.P-A.*, " Analysis Terminable and Interminable," 1937, vol. XVIII, p. 402.

whole subject of the analyst's relation to his work and its effect upon him. In any such investigation the following factors would have to be distinguished: (*a*) the stimulation of unconscious conflict through analytic work; (*b*) the effect of overwork; (*c*) the effect of emotional frustration, e.g. of sacrificing too much energy without commensurate recompense; (*d*) the degree of flight into unreality encouraged by concern with other people's problems both real and phantastic. In particular, the interplay and interchange of super-ego rôles is worthy of investigation. Both analyst and patient are in a sense " on their best behaviour," and this unnatural state is bound to provoke super-ego reactions.

4. THE NEUTRALITY OF THE ANALYST (LAY-FIGURE CONCEPT) (Q.7 (1)). *Do you ever admit to the patient the possibility of being wrong? Do you ever admit to the patient the possibility of " not knowing " ?*

This is another of the few questions which produced unanimous replies. All answered in favour of admitting fallibility to the patient. Only one special reservation was made to the effect that it may be unwise to do this with patients whose deep anxieties are so strong that they still " need " to feel the analyst is omnipotent.

In this case the questions were badly selected. On the one hand, it is not easy to confess that one never admits error to the patient and, on the other, there is no doubt that many hysterical and depressive patients react strongly to the idea of impotence or weakness on the part of the analyst. This last situation is, of course, one that calls for transference interpretation.

Obsessional patients are quick to recognise uncertainty on the part of the analyst. This has some bearing on the length of interpretations (*q.v.*, p. 16) : long interpretations more than any others arouse suspicion of weakness.

5. CO-OPERATIVE NATURE OF ANALYSIS (Q.7 (2)). *Do you lay emphasis on the co-operative nature of analysis as distinct from the phantastic co-operation demanded in transference ?*

A majority emphasise co-operation but a few are definitely against it. One suggested that emphasis on co-operation might lay a burden of guilt on the patient, making him feel responsible for failure.

6. FRIENDLY ATTITUDE *VERSUS* PROFESSIONAL INTEREST (Q.7 (3)). *Do you believe in giving some indication of a positive friendly attitude as distinct from the friendliness implied in " professional interest "?*

One-third replied " Yes," another third, " No," i.e. should not exceed " professional " friendliness. Of the affirmative replies one insisted that the analyst must not be afraid to be human.

7. REAL *VERSUS* " SHADOWY " ANALYSIS (S.Q. C(8)). *If you have no fixed or rigid practice in these matters, what indications do you follow ? For example, even if you hold in general that the analyst should be a neutral or shadowy figure, are there instances where you believe he should be more of a " real " figure for the patient ? If so, in what respects ?*

Two answers only were against departure from neutrality under any circumstances. About one-third favour neutrality with some qualifications, e.g. " unless a kindly super-ego rôle is necessary in a crisis " and " ego must feel the analyst a stand-

by." Another third insist that the analyst must be throughout human and real. Some of these think that the opposition between neutrality and reality is false and that the conditions of analysis allow the patient ample opportunity to prove the analyst's reality without the latter abandoning his neutrality by any form of self-assertion. A small minority, however, believe that the analyst should deliberately emphasise his " reality " in some cases of acute conflict, where the ego is weak. One recognises the need that may arise for extra ego support, but thinks this can best be given by directing analysis to the most urgent problems.

8. RESTRICTION OF SPONTANEITY OF ANALYST (S.Q. C(9)). *How far should the spontaneity of the analyst be restricted? Are there any dangers or drawbacks in such restrictions?*

Only twelve answers were received. Of these, six realise some restriction (e.g. of speech) is necessary, but find it difficult to draw the line between desirable and undesirable restraint. What must be avoided is the establishment of an unfavourable " inhibited " atmosphere. Five feel that the analyst ought to be able to trust himself to be spontaneous, and that restriction is often defensive and therefore to be distrusted. Only one sees no dangers in restriction.

So far the answers to these questions on the neutrality of the analyst provide a useful " control " for the opinions expressed on the subject of " reassurance " (*q.v.*, p. 44). These latter appear to have been fairly consistently held, since there is also a definite consensus of opinion in favour of routine " neutrality " but in favour of the " real human " as against the " shadowy " analyst. Nevertheless, there is a slightly stronger conviction in favour of the " real human " than there was in favour of the " reassurance." Although a policy of reassurance is not incompatible with neutrality of the

analyst, the question arises whether for most patients strict neutrality is not really an intimidating attitude. At the least it may represent an incongruous aloofness. If that be so, the real human attitude is essentially reassuring. If it be reassuring, at what point does it lose its legitimate effect and become a transference instigator ?

It was unfortunate that information on the " spontaneity " of the analyst was so scanty. Commencing analysts are constantly harassed by this problem and tend to solve it by swinging between rigid caution and an expansive plunging. Generally speaking, the more experienced the analyst the more spontaneous he is. The risks of spontaneity lie mostly in the selectiveness of the analyst's unconscious mechanisms and conscious theories and partly in the possibility that the analyst may feel guilty over having been spontaneous.

All these points require further investigation. In the original question no definition of " professional interest " or " friendliness " was given, and no doubt there is a considerable overlap between " professional friendliness " and a " human attitude." For many physicians " professional interest " is confined to a benevolent scientific concern with symptoms, an attitude which is frequently infuriating to patients. In the second place, although it is true that the conditions of analysis permit ample opportunities of recognising many reality aspects of the analyst, the important point is : Does the analyst *admit* to the patient the accuracy of any just observations or guesses about him ? The next question touches on this subject but only in so far as " allowances " have to be made

for the patient's real estimations. It does not raise the point of " admission."

In her paper on analytic reassurance * Melitta Schmideberg suggested a number of reasons why it may be desirable for the analyst to appear a more real figure for the patient: (*a*) that it may reassure the patient, (*b*) that it may intensify emotional conflicts, thus bringing them out, (*c*) that it may counteract unreality feelings in the patient, and (*d*) that it may gratify or permit some of the patient's scopophilic impulses and criticisms. The routine of the analytic situation contains many obsessional elements which may fit in so well with the obsessional systems of the patient that they escape analysis (in marked contrast to the reaction of patients who are frightened by analytic routine).

9. PATIENT'S " READING " OF ANALYST (S.Q. C(10)). *How far do you think the patient's unconscious capacity to read the analyst's psychological tendencies should be allowed for? (This involves the problem of how far the transference situation is solely a projection or displacement on the part of the patient, or how far it is a (conscious, unconscious) recognition of the real (conscious, unconscious) attitudes of the analyst.)*

Quite a large majority appeared to believe that patients observe or read the analyst's unconscious. There was a tendency to regard the transference as a mixed product, but to estimate its " projection " components as variable but high in proportion to the " perceptual." One said 80 per cent. Several remarked that the projection elements are the only ones of fundamental importance for analysis.

10. ADMISSIONS TO PATIENT (S.Q. C(11)). (*Sample problem arising from the foregoing.*) *Would you admit change of mood, anxiety, or personal illness to the patient?*

* *loc. cit.*

Most of those who answered are willing to make admissions to patients, in answer to questions, confirming observations or under pressure of necessity, e.g. to account for missing appointments. A small minority do not, as a rule, confirm or deny patients' suppositions, or make admissions. Others regulate their admissions by consideration of the probable effect on the patient.

11. COMMUNICATION OF ANALYST'S PERSONAL OPINIONS (S.Q. C(7)). *Do you communicate personal opinions* (e.g. *cultural, social, or political views*) *?*

The majority do not communicate personal opinions (two " not consciously," " unless by implication "). One makes a special point of not doing so. Others do so " rarely " or " at times." One answer was a definite " Yes."

These last questions are directed more specifically at the problem of counter-transferences and what is to be done about them. There are two main aspects of counter-transference. Corresponding with the patient's " floating transferences," which operate most obviously at the beginning of analysis, there are on the analyst's side spontaneous or " floating counter-transferences " and no doubt these tend to continue unobtrusively throughout analysis. There are also " acquired counter-transferences " which are essentially reactions to the patient's material. The latter variety of counter-transference is obviously a matter for self-analysis on the part of the analyst. There is not the same certainty that the former, more spontaneous, reactions are equally amenable. No doubt they can be controlled, but the spontaneous likes and dislikes characteristic of the individual analyst are not very likely to be reduced.

As Freud has said, even the deluded paranoiac does not project aimlessly; he has an appreciation of the essential hostility lying behind the indifference of the stranger. Despite the one-sided intimacy of the analytic situation, the neutrality of the analyst makes him a "stranger" to the patient. The practical point is that on a number of occasions the patient's projections or displacements are in a sense accurate diagnoses. If this situation can be dealt with by self-analysis, good and well. If not, how far should the analyst just ignore the diagnosis or how far should he admit frankly the possibility of the patient's reading being accurate? Further, if he is ready to admit it, how far should he avoid a rigid attitude of self-effacement which is evidently not being taken by the patient at face-value? The difficulty in all such problems is: What are the limits of natural expression compatible not only with effective analysis as a whole but with effective analysis of the immediate situation provoking the patient's reaction?

A specially difficult situation arises with patients to whose personality the analyst may feel antipathetic. This reaction can activate both the spontaneous and the acquired varieties of counter-transference. Especially when such patients produce, as they frequently do, a prolonged series of "negative" responses, the situation is a testing one. And the analyst may respond by keeping rigidly to a conventional analytical attitude, or by exhibiting a little more friendliness and openness than is usual. On the other hand, cases have been reported where, in face of a negativism calculated to arouse hostility in the most long-suffering individual, a frank disclosure of

this counter-reaction had an apparently good result. The result was no doubt due to the fact that the patient's aggression was held in check without any sense of humiliation and without employing any moralistic attitudes. But since patients sometimes regard the analyst as a helpless child, the fact that the analyst did not show any sign of manifest or latent fear, in particular that he was not afraid to disclose his own hostile response, no doubt contributed to the improvement in the analytic situation.

A number of other interesting points arise in this connection. For example, how far does the analyst's refusal to admit the reality of some of the patient's observations lead to theoretical distortion or to actual over-emphasis of phantasy interpretations. If the patient roundly states that the analyst is dressed in a slovenly fashion, it is always possible to interpret this as a sign of anxiety lest the parent should be impoverished. But if the analyst happens to be slovenly in dress habit the interpretation might have more effect after a frank admission of the justness of the criticism. Even in the case of signs of illness exhibited by the analyst there are some risks in refusing to admit the accuracy of the patient's diagnosis. The patient may react to the denial as a repetition of parental denial of the right to observe, and if the observation is merely interpreted in terms of infantile sadism, the patient may respond to this interpretation with an increase of guilt feeling.

There is, however, a clear consensus of opinion against communicating " personal views " of a general sort. Closer investigation might show under what circumstances and to what extent this attitude (amongst

others) might be relaxed. If it is disadvantageous analytically to relax it, in what ways can the analyst be spontaneous and at the same time analyse his patient's transferences without producing an impression of defensive or contemptuous aloofness ? The following three questions were intended to explore these possibilities.

12. TECHNICAL INFORMATION OR ORIENTATION (Q.3 (2)). *Do you usually act as a source of information (reality) on* (a) *sexual,* (*b*) *non-sexual subjects ?*

The great majority act as sources of information, but only one-third appear to be untroubled about doing so. The others offer various explanations (excuses), the main justification being the removal of impeding ignorance in the patient. One gives no sexual information, another no general information.

It was noted that the tendency to analyse questions was general. For instance, it was suggested that it is a mistake to analyse questions where there seems to be reality justification for giving information, on the ground that too constant analysis of questions not only induces immunity or refractoriness in the patient but is a sign of undue anxiety on the part of the analyst.

13. TELEPHONING (S.Q. C(4)). *Do you allow telephone conversations ?*

A majority allow telephone conversations, but may try to keep these short or limit them to practical matters. Quite a large minority definitely discourage or do not allow such conversations. Clearly no one encourages them.

14. READING LETTERS, ETC. (S.Q. C(5)). *Do you read letters, look at photographs, etc., brought for your inspection ?*

Most of the answers were in the affirmative. None were wholly negative. A considerable number aim at some sort of compromise such as getting the patient to read a letter aloud. One said he "analyses impulse first, so rarely looks." The situation resembles that about telephone conversations (C.4 preceding). The practice is not positively encouraged, but is treated with varying degrees of tolerance according to circumstances.

It has to be admitted that here again the answers are not very helpful. It is clear that a desire exists on the part of the analyst to avoid giving his patient an impression of rigidity and within limits to present a friendly co-operative appearance. On the other hand, it seems not only that analysts are frequently uneasy about ways and means of giving this impression but that they have not worked out the principles by which such reactions should be regulated. Actually the subject is one that requires a good deal of clinical investigation before guiding principles can be laid down. Experience of consulting work brings out the fact that many minor problems of analytic routine are of major importance to the spontaneous rapport that is of such consequence in consulting practice. With hysterical and psychotic cases, in particular, it can be observed that substitutes for consultation such as telephoning, letter-writing, etc., are sometimes of decisive importance. Paranoid types often preserve a sort of uneasy equilibrium by writing frequent letters to a consultant whom they have never seen. Telephoning is another of those spontaneous plans of campaign on the patient's part which seem to have a stabilising effect. Observations of this sort, reduced to scale, would perhaps enable decisions as to analytic routine to be arrived at

on systematic clinical grounds. The possibility of using the telephone for purposes of interpretation in urgent cases was not considered. In the Supplementary Questionnaire an attempt was made to bring out underlying principles by enquiring about a simple test problem of routine (see below).

B. A Preliminary Problem in Routine

1. SMOKING : ANALYST (S.Q. A(1)). *Do you smoke during work?*

There appeared to be a few more smokers than non-smokers. Among smokers, the majority smoke habitually, a few " rarely " or " according to circumstances in any given case." Of the non-smokers, two were non-smokers outside analysis also, and three very moderate smokers. Two remarked that they have no principles about it but simply find they listen better if they do not smoke.

2. SMOKING : PATIENT (S.Q. A(2)). *Do you permit (or invite) patients to smoke?*

A majority permit smoking, but some qualify their permission, e.g. convey " resistance possibilities." Only two actively discourage smoking. On the other hand, only a small minority ever invite patients to smoke. The majority do not do this as a rule. One reports that some of her patients smoke her cigarettes, some occasionally, some habitually, and in any case is always prepared to provide cigarettes for those patients who forget to bring their own.

3. SMOKING : INDICATIONS IN ABSENCE OF FIXED PRACTICE (S.Q. A(3)). *If you have no fixed practice, what indications do you follow :* e.g. *with (a) what types of case do you either smoke yourself or permit (invite) them to smoke ; (b) what type of situation in any one case?*

4. SMOKING: BASIC PRINCIPLES OF FIXED PRACTICE (S.Q. A(4)). *If you have a fixed practice on what general principle is it based?*

Many failed to answer these questions. Of those who did, a small majority refrain from smoking if the patient objects or is rendered anxious by it. One smokes less indiscriminately with women than with men and never with male paranoids. Two stated that they are guided by the principle of not interfering with the patient's habits. Some permitters interfere if they feel the habit is becoming too successfully " defensive." Several permit, encourage or invite where the action seems a sign of progress on the patient's part or in acute anxiety. One mentioned the wealth of information which may be obtained from the details of a patient's smoking habits as an argument against interference.

If failure to answer questions is any criterion, the " test " problem must have produced a certain amount of doubt in the minds of contributors. On the whole, the replies represent the same differences of view as were apparent in the answers to more general questions on the subject (Section A). They indicate, however, that the issue is not solely one of transference and counter-transference, nor of the indications for reassurance. There is a third factor to be assessed, viz. the analyst's views on the advisability of " abstinence " in analysis. In the original Questionnaire this subject was approached *via* an enquiry about " Active " Devices. The report on this follows immediately.

C. Active Therapy

1. ACTIVE DEVICES (Q. 5). (*To avoid lengthy definition, this may be taken in the " Ferenczi " sense.*)

Preliminary. *Apart from laying down the association rule do you give any general or specific recommendations re suspending personal habits (masturbation, etc.) before analysis commences?*

During analysis. *Do you employ prohibitions, or* positive injunctions: (1) *Aimed at symptom habits, phobias, obsessions?* (2) *Aimed at psycho-sexual habits, masturbation, perversion, fetich, intercourse—* (*a*) *marital,* (*b*) *extra-marital, promiscuity? If practice varies, give indications for policy in different cases.*

Objection to preliminary interferences (apart from association rule) was unanimous. One analyst does not always recommend even free association. A few, at the outset, recommend that there should be no drastic changes in occupation, etc., during the course of the analysis. The general tendency was also strongly against the use of "active" devices during analysis. A minority use them occasionally in relation to symptomatic, sexual and social habits. Of those who use positive injunctions, one tends to enjoin exhibitionistic displays. Another always tries only to reinforce ideas the patient himself is already playing with and to avoid supporting self-imposed restrictions of a masochistic nature. Those who use prohibitions usually direct them at sexual or social habits rather than at symptom habits.

These replies show very clearly that Ferenczi's "active devices" are fading out of technique in England. It would perhaps be more accurate to say that from the beginning there was considerable doubt as to applying them. Certainly, a great number of analysts refrained from doing so, and those who did apply them have now almost given them up. Nevertheless, the replies are not altogether satisfactory. The original stimulus (or possibly, the original justification) for Ferenczi's work lay in the recommendation made by Freud that analysis should be carried out in a state of abstinence. It has to be remembered also that the number of self-imposed abstinences carried out by patients as a result of being in analysis is quite remark-

able. It seemed desirable to investigate this cardinal problem of " abstinence " more directly and a definition was asked for in the Supplementary Questionnaire

2. ABSTINENCE (S.Q. A(5)). *What do you understand by the term " abstinence " ?*

Such definitions as were obtained were very various. A majority, however, evidently understood the term to imply deliberate abstention by the patient himself from id-satisfaction (one stipulated " at the behest of the ego, not the super-ego "). To this majority then, abstinence means a state of self-imposed deliberate endurance of id-tension by the patient. Quite a large minority, however, give definitions or illustrations implying deprivation by the analyst. To two, analytic abstinence is transference frustration. One definition ran, " a condition in which reassurance and libidinal gratification are sufficiently unobtainable to create a demand that stimulates the analytic work." One is chary of " Don'ts " and prefers " Do's " but has sometimes suggested reduction in number of repetitions of obsessional performances during the analytic hour.

The most that can be said of these replies is that they leave the issue very much where it was. More painstaking enquiry into this matter of " abstinence " is indicated. A number of other factors should be taken into consideration. For instance, although it is evident that prohibitions are not now generally employed, it is always possible that the theoretical views held by the analyst can amount in the long run to something very similar. Should the analyst believe that no analysis is complete which does not go thoroughly into depressive and masochistic layers, and if it be a characteristic of depressive masochistic mechanisms that they induce a degree of self-inhibition, then emphasis on these views in analysis would in

practice favour the development of a self-inhibiting phase in the patient.* Similarly, if the analyst has hard and fast views on the pathological significance of manifest homosexuality and if he gives expression to these views, this is tantamount to saying to the patient " I shall not regard you as well (or, I shall not let you go) till you give up this practice and substitute hetero-sexual practice." This could legitimately be called a " crypto-Ferenczi " policy (see The Relation of Theory and Practice," p. 132). The same possibility does not apply to symptoms that inflict suffering on the patient because there is common consent (conscious if not always unconscious) that it is desired and desirable to get rid of them.†

* How continuously suggestion is at work during analysis and how great the need is for objective enquiry into the mode and degree of its operation were points raised by Melitta Schmideberg in her contribution to Discussion, February 16, 1938. See also her paper " After the Analysis " (*Psycho-analytic Quarterly*, 1938, vol. VII, p. 135) for the interaction of suggestion and implied moralistic views on the part of the analyst.

† Edward Glover writes : " Examination of the effect of inexact interpretation in analysis focusses our attention on the possibility that what is for us an incomplete interpretation is for the patient a suitable displacement. By virtue of the fact that the analyst has given the interpretation, it can operate as an ego-syntonic displacement system (substitution-product, symptom)." He believes that in suggestion therapy " the suggestionist plays the patient at his own game of symptom formation " by offering him a set of affectively toned ego-syntonic ideas in place of his ego-dystonic symptom. He implies thereby that an inexact interpretation given in analysis may operate as an (unwitting) suggestion. " The Therapeutic Effect of Inexact Interpretation : a Contribution to the Theory of Suggestion " (*I.J.P-A.*, 1931, vol. XII, p. 397).

CHAPTER VII

TRANSFERENCE AND ROUTINE—II

D. Appointments and Time-Table

1. LENGTH OF SESSIONS (S.Q. C(1)). *Do you always keep strictly to the same length of session?*

The majority keep to the same length of session as a rule (inevitable minute variations). One " strictly to a second." Others " not precisely," " never more than five or ten minutes over," " may prolong in crisis but only for a short time."

2. TIME-TABLE (S.Q. C(2)). *Do you allow a gap in your time-table to permit of extending sessions?*

Only four allow such a gap, none of them more than ten minutes.

3. REPEAT SESSIONS (S.Q. C(3)). *Do you see patients again in the same day? If so, for long or short periods?*

A majority do not see patients twice in the same day unless for some reason a double daily session has been agreed to as routine. Nevertheless, many analysts have seen patients twice in serious emergencies. The deciding factor is evidently " urgency."

These are by no means to be regarded as banal problems. Precisely the same principles are involved as in the previous section. First, the significance of routine arrangements as instigators of transference

phantasy (unconscious and pre-conscious) and as outlets for counter-transference reactions ; second, the significance of what might be called " reality-fringes " of the analytic situation for the (pre-)conscious layers of the patient's mind (time and money reactions are after all the chief reality situations by which the patient can test the analyst's behaviour) ; third, the part played by unconscious and conscious reactions to these routine procedures during crises in the analysis. This last point touches again on the problem of reassurance as an auxiliary to or substitute for interpretation during crises. How far does the total analytic situation act as a current support to the patient, enabling him to renounce the more defeatist policy of regression and to endure a frequently painful reversal of his mental defences through interpretation? Although the matter was not raised very much in these replies, there is no doubt complete agreement that routine and, in particular, time-routine is an extremely valuable stimulus for transference phantasy and that it provides a constant opportunity for ventilating unconscious phantasy. Archaic phantasy attaches itself very readily to the immediate " circumstances " of analysis. Baby reactions to time of a depressive or paranoid type are easy to detect and are much more intractable than hysterical outbursts on the same subject. Obsessional cases are superficially easy but deeply ambivalent about time. So much can be agreed. On the counter-transference side a great deal needs to be said. The fact that analytical routine can be read in terms of the analytic situation tends to make analysts gloss over the fact that many of their routines depend more on their own convenience than on the

patient's needs. The analyst's reaction to length of sessions, overlapping and repeat sessions obviously depends to some extent on the exigencies of his own practice. And these in turn often have a direct relation to his financial needs or values. Should these time and money factors be specially important, the analyst might unwittingly be tempted to rationalise them. And patients for their own reasons (need for love or preferential treatment or desire to avoid resentment) are ready to accept such rationalisations. To put this another way: it would be interesting to know what the general analytic policy about time would be if all analysts were financially independent and spread half the usual number of cases through their working day. With regard to relaxations of time rules, the reply that these depend on the " urgency " of the situation is not entirely satisfactory. In the first place it depends on what use is made of the extra time, e.g. whether further interpretation is given or opportunity for further working-through or more complete catharsis, or whether the relaxation is simply a human gesture that has a reassuring effect. In this connection it must be remembered that for many years analytic practices have in the main dealt with ambulant cases of psychoneurosis. On the whole (and this applies to lay analysts in particular), analysts have led in the past a cloistered professional life. Hence the tendency to stereotyping views on routine. The problem of time is naturally more acute for those who treat a number of genuine psychotics as well as psycho-neurotics, more especially if at the same time they have to meet the demands of a consulting practice. On the one hand, their time-tables are ex-

tremely congested, and do not allow much overlapping and on the other psychotic cases, more than any other type, necessitate elasticity in handling. In arriving at guiding rules as to time routine it would be better in the first instance to exclude the factor of " urgency." Finally, it would be interesting to consider how far strict time routines increase the danger of making the analytic situation an obsessional one. Where both patient and analyst are obsessional in type, this aspect of the analytic routine is unlikely to provoke any comment on either side. It may, indeed, be a source of reassurance to both. On the other hand, it seems to be the custom to interpret the patient's reactions to the analyst's time-table only when these are of a negative type.

E. Fees

1. PAYMENT FOR NON-ATTENDANCE (Q.8 (1)). *Do you have a standard rule re payment for non-attendance? Do you keep to it?*

A large majority have a standard rule relating to payment for non-attendance but only about half adhere to it. There is a general tendency to remit or reduce when absence is due to illness or other " real " cause.

2. RAISING FEES (Q.8 (2)). *Do you ever raise fees during analysis? If so, when?*

Only one-third of those who answered had raised fees during the course of analysis. There seemed to be general agreement that this should be done if a patient who is being treated at reduced terms becomes able to pay more or is found to have understated his resources. One stated that he not only believes in this on principle but has in fact found it helpful in practice.

3. EXTRA-MURAL CONTACT (Q.8 (3)). *Do you accept presents from patients? If so, on what system?*

No one accepts large presents or money offerings. In some cases acceptance was said to be conditioned by the patient's means, but these conditions vary. It has been noted that some patients who cannot afford full fees are more inclined to offer small gifts. Acceptance is more often determined by reference to the analytic situation, e.g. where giving is a sign of progress, where snubbing is undesirable, etc. The majority evidently do not receive gifts gladly ; there is a marked tendency to analyse the motives of giving with a view to curbing gifts. There emerged an interesting difference of opinion *re* the significance of gifts. One analyst thinks gifts a sign of counter-transference. Another thinks " few gifts " a sign of a defect in the analyst. Apart from some reports of " few gifts " there is no other information about the giving reactions of special types of case.

This is a section which might have been expanded with considerable advantage. Useful questions would have been : Do you reduce fees, if so, under what conditions ? Do you allow fees to run on credit ? Do you accept payments in advance ? Apart from hospital practice, do you analyse patients gratis ? If a patient becomes insolvent do you continue the analysis in private without fee or do you transfer the case for clinic treatment ? Or again : Do you lend money to patients ; if so, under what circumstances ? Are the amounts large or small ? As it is, the replies bring out the fact that in the matter of fees there is much more general hesitation to apply rules strictly than there is in the case of time arrangements. This hesitation is probably due in part to more acute conflicts aroused in the analyst by money than by time. The

replies to the questions on " gifts " are even more interesting in this respect. It seems that analysts can take a more definite stand for or against " reassurance " so long as these problems are discussed solely in terms of verbal interpretation and verbal reassurance. Some of those who were against verbal reassurance were, nevertheless, not against accepting small gifts, i.e. they were prepared to make concessions when the issue became more direct and personal. But it can be argued that if one accepts small gifts in order not to appear inhuman the effect is not simply due to the avoidance of snubbing : it is reassuring to the patient. But if reassurance is justified at one point what are the proper or expedient limits of the process ? Incidentally, it is worthy of note that answers to these questions of detail were much less free and voluminous than when the issue appeared to touch more obviously on technical principles.

F. Social and Family Contact

1. SIGNIFICANCE OF SOCIAL CONTACT (S.Q. E(3)).
Is the significance of social contact in analysis of secondary importance ?

Some confusion arose here between the social contact inseparable from analysis and extra-analytical contact. There seemed to be a general feeling against extra-analytical contact during the course of analysis, chiefly on account of possible " transference " complications (on *both* sides). The importance of such contact derives chiefly from transference effects. Two analysts, however, incline to regard contact as of secondary importance provided its effects are adequately analysed.

2. BEGINNING AND END OF SESSIONS (Q.9 (1)). *Do you shake hands before and after? Do you use small talk? Do you permit small talk? Do you lend books, etc.?*

Hand-shaking practice appeared to vary. One-third regularly shake hands before and after, one-third definitely do not. The remainder have no fixed rule but vary with patients.

Similar variation exists in regard to small talk. One-half use it occasionally but do not as a rule initiate it. One-quarter do not use and discourage it. The remainder vary. No one refuses to allow it but a few discourage it rather pointedly. There is a general tendency to analyse small talk subsequently.

Two-thirds lend books sometimes or would do so if asked. One-third do not do so.

3. EXTRA-MURAL CONTACT (Q.9 (2)). *Do you meet your patients socially? Do you avoid meeting patients socially? If so, why?*

All tend to avoid meeting patients socially, though one analyst suggested that it might be useful in some psychoses. The reasons given related to possible transference and counter-transference complications (cf. S.Q. E.4). One succinctly replied " Because it is a nuisance to both parties."

The investigation of procedure regarding social contact was interesting for a special reason, namely, that in a number of instances it is possible to " control " the responses of the analyst. Under existing conditions of training candidates, it is sooner or later inevitable that the training analyst should make a degree of social contact with his analysand. This is at first limited to a professional form (lectures, seminars), but later some degree of ordinary social contact can only very rarely be avoided. Since, however, a didactic analysis does not

differ from a therapeutic analysis except in so far as it is supposed to be deeper and more thorough, we may presume that some analyses can be carried to an unusually satisfactory conclusion under conditions which are fairly generally avoided in ordinary practice. Indeed it is sometimes held that the unusual stimulations are not to be regretted because, happening in the later stages of training analysis, they provide extra tests which the candidates ought to be able to stand. But this is to concede some advantage, albeit under special circumstances, to the " reality-fringes " of analytic contact and to some doses of " extra-mural " contact. These advantages can only be : giving special opportunities for transference interpretation (i.e. a mild kind of " active therapy "), or transference opportunities plus reassurance, or reassurance irrespective of its transference interpretation.

Despite all these considerations, one should not lose sight of the fact that problems about time, fees, or social contact are not just professional matters involving interpretation, and/or reassurance or stimulating transference and counter-transference. The analyst, like any other professional person, is entitled to secure as much privacy as the exigencies of his practice will allow. Psycho-analysis is not a very well-paid specialty ; it is an exacting profession and takes up a good deal of time. The amount of psychic contact made with a small number of cases is infinitely greater than in any other branch of medical science. And as one of the contributors justly suggested, extra-mural contact may be a nuisance to both parties. On the other hand, very few medical practitioners have such regular time-tables as analysts have, are so free from night or week-end calls or

can take such lengthy holidays. In ordinary general practice " urgency calls " have priority over routine treatments. " Repeat visits " depend on the state of the patient and not on transference rules. In short, although the work in most branches of medicine is easier and less continuously exacting, there is greater obligation to pay immediate attention to ups and downs in the patient's condition. It so happens that a conventional analytic practice, dealing mostly with plain-sailing neuroses, inhibitions, perversions, and character difficulties, can be run according to traditional analytic rules. But the fact that it can be run in this way is in itself no sanctification of the rules. Rules must be justified by a sound combination of accurate theory and good empirical observation (see also Psychoses, p. 128).

4. SOCIAL CONTACT AFTER ANALYSIS (S.Q. E(4)). *Do you desire to have any social contact with the patient after the completion of the analysis ?*

A fair number replied that they sometimes wish for after-contact, or would permit it if the wish were mutual. Several mention that it seldom actually occurs. Others replied " No," or " Hardly at all." One would make no attempt to avoid it if it occurred naturally. One reported it as having occurred without disadvantage. One or two seemed to be ambivalent or indifferent about the matter. Another urged the application of ordinary reality criteria.

5. FAMILY CONTACT : INTERVIEWS (Q. 9(3)). *Do you interview members of family (a) with, or (b) without, the patient's knowledge ?*

All see members of the family, mostly unwillingly, and at the patient's request. With few exceptions (severe psychoses, children) interviews are arranged with the knowledge of the patient.

6. FAMILY CONTACT : ANALYSIS (S.Q. G(1)). *Do you favour analysing two or more members of the same family (at same or different times) ?*

The answers revealed a clear majority against analysing members of the same family, at any rate at the same time. A minority would analyse them at different times, although some of these say definitely " not husband and wife." Three, however, replied unequivocally " yes," and one of these stated she had found it practicable to analyse a husband and wife. Another who has tried this says it has some advantages but the disadvantages are infinitely greater.

7. ANALYSIS OF FRIENDS (Q.9 (4)). *Do you analyse patients who have emotional ties (friendships, etc.) to your own friends or family ? Or who are related to or friendly with individuals in any way dependent on you ? On these matters have you any guiding principle ?*

Over two-thirds avoid analysing friends unless there is some extremely cogent reality reason for doing so. A very small minority are not convinced that analysis under such conditions is so unfavourable as is usually taken for granted. Some of these report successes with analysands on the fringe of acquaintance.

The fact that there is little divergence of views on this group of questions stimulates some interesting reflections. The questions are complementary to those on social contact. Whereas in the latter case the point was whether such contacts might promote or retard progress in difficult phases, the issue of family contacts is concerned solely with conditions likely to retard progress or make analysis more difficult as a whole. The transference points involved are comparatively simple, e.g. the stimulation of such phantasies as " conspiring with the parents " and " jealousy

triangles." In the case of analysing husbands, wives, friends, although the conspiracy idea is phantastic, the jealousy reaction is to some extent justified. And obviously where situations of unfaithfulness existed or where divorce problems suddenly arose, the difficulties of analysing both husband and wife would be insuperable. The consensus of opinion in favour of avoiding such conditions raises the following question : if it is proper to avoid contacts which increase obstructive transference phantasies or realities, are there any varieties of contact which would promote the smooth working of analysis ? The question of analysing persons who are friendly with individuals dependent on the analyst did not produce any helpful opinions. It should have been put more clearly. Training analyses show that when the analysand feels dependent professionally on the opinion formed by the training analyst as to his vocational qualifications, violent and ambivalent transference phantasies are aroused. The situation sometimes promotes a spurious amelioration of symptoms. Even when this factor of indirect economic pressure is absent, the situation may lead to a slightly spurious advocacy of the scientific views of the training analyst (see also pp. 3–6). Similarly, when individuals in any way (emotionally or economically) dependent on the analyst make a point of press-ganging their friends to be analysed by him (and this is a fairly common practice of ex-analysands who finish their analysis in a state of concealed negative transference), the situation may well give rise to a spurious " readiness to get well."

CHAPTER VIII

TRANSFERENCE AND ROUTINE—III

G. Sex of Analyst

1. SIGNIFICANCE OF SEX OF ANALYST (S.Q. E(5)). *What significance do you attach to the sex of the analyst?*

A majority replied " None," " Not much," " Anybody ought to be able to analyse any case," etc. A respectable minority, however, said " some," and several of these feel that the matter is one for research. (Cf. S.Q. E.3.)

2. SEX PREFERENCE AND CLINICAL TYPES (S.Q. E(6)). *In what cases would you prefer to recommend a male or female analyst?*

About half of those who answered incline to send homosexual patients (excluding those in whom definite symptoms of paranoia are present) to analysts of the same sex. The other half incline to recommend sending a patient to an analyst of same sex inasmuch as the latter represents the less dreaded parent. The initial stages of analysis are rendered easier in this way, especially where anxiety and animosity are very pronounced. One analyst doubts if a woman ever works through the deepest anxieties connected with her mother with a male analyst. Another even thinks that a woman analyst might be best for both sexes on account of the primary importance of the mother.

3. CHANGING ANALYSTS (S.Q. E(7)). *Do you favour changing analysts for therapeutic purposes or changing to analyst of opposite sex?*

The majority do not favour changing analysts as a rule. They might under exceptional circumstances, e.g. complete standstill in analysis, or technical blunders (especially transference mistakes). One or two have " no opinion " or are " uncertain " and a few consider it possible that some cases might benefit.

An element of inconsistency can be detected in these answers. If the sex of the analyst is of no importance why take it into account in cases of manifest homosexuality ? Alternatively, if one takes it into account in cases of manifest homosexuality, why not also with clinical types in which massive disturbances of unconscious homosexuality can be presumed ?

A most interesting suggestion is made by two contributors : that perhaps no thorough analysis can be made by a male analyst because of the primary importance of the mother in both sexes. This is possibly a reverberation from earlier clashes on the subject of unconscious sexual development, e.g. that male analyst's views on female development are suspect, and vice versa. But the conviction has deeper roots and involves a number of interesting possibilities. In the first place, it implies a certain scepticism as to the validity of transference phenomena. For if transference is mainly a phenomenon of displacement and projection and if there is little need to admit to the patient the reality aspects of transference, then clearly the sex-personality of the analyst would avail little. Actually there is abundant clinical evidence that " mother " situations are worked through in relation to male analysts and " father " rôles assigned to women, as the patient's unconscious position demands. Further, if " mother " feelings are not capable of being

adequately or completely switched to father or brother figures in the course of infantile development, it would follow that " father " feelings would vary in importance with the actual rôle played by the father in infancy, i.e. with the time, nature, and constancy of his appearance in early life. Here again, there is abundant evidence from actual analyses that the absence of the father may be of greater unconscious significance than the presence of the mother. Further, if the sex of the analyst were of major importance in the analysis of all cases where the symptoms could be related to early conflict over the mother-relation it would follow that for all patients whose fixatives and symptoms lie in later infancy, that is to say, at a time when the father imago has, for whatever reasons, acquired preponderating importance, either as a loved or hated object, a male analyst would be essential for the greater part, if not indeed, throughout the analysis. According to this argument it would be positively unprofessional for women analysts to analyse the œdipus complex in women patients ! In short, the suggestion would appear to be subjectively determined rather than based on evidence. And it is interesting to note that the analysts who brought it forward (thereby running counter to the concepts of transference and displacement) were amongst the most insistent on the importance of transference analysis. Perhaps the most interesting point arising from the suggestion is that beliefs of this sort involve a tacit approval of " active " devices, e.g. that to work through difficulties connected with the " mother," the patient should be analysed by a " real " woman. And, here again, it is to be noted that the sponsors of the suggestion were not only

against the adoption of " active " devices, but were strongly in favour of endopsychic as distinct from environmental factors in psycho-genesis.

Further, the suggestion that because of their sex women analysts are better than men analysts implies that *reliving* the pre-genital œdipus situation is as important as, if not indeed more important than, interpretation of it. And this in turn implies that modification of the severe maternal super-ego is achieved through the reassurance of reliving the situation. This would be a form of reassurance radically different from the policies of temporary reassurance previously discussed. And it would imply that it is desirable to reinforce the homosexual attitude of women patients and the child-like attitudes of men patients. But this would be a tacit encouragement of regression which logically would involve a transfer of the patient to a male (father or brother) analyst in order to encourage progression.

The whole subject bristles with unresolved professional and emotional problems. It was suggested earlier (see p. 25) that perhaps differences in the form, timing, and quantity of interpretation might have some relation to the sex of the analyst. But the suggestion was qualified to the effect that the decisive factor is not specifically sexual but concerned with counter-transference in general. In unofficial discussion the idea has sometimes been mooted that women make better analysts than men because mothers are more in contact with children than fathers. But this is to ignore rather pointedly the significance of the bisexual constitution, and is based in the long run on a " mother idealisation." Besides, the argument

is two-edged and might have awkward repercussions.*

The question of *changing* to an analyst of different sex raises a very similar problem. It would seem that, as analysis spreads, occasions of changing analysts become more frequent. The issue also touches on the validity of " active " transference instigation. There is no doubt that the *order of presentation* of psychic material is frequently (if not invariably) affected by the sex of the analyst, at least in the opening phases of treatment. But beyond that, and confirming the view that transference is not limited by conditions of sex differentiation, there appears to be little agreement and the answers throw no light on how and under what circumstances advantage can be taken of these facts. Experiences gained by consulting analysts might throw some more light on these problems. The mere fact that analytical consultants before recommending any particular analyst are ready to take into account the patient's conscious predilections is of some significance. It seems probable that some types of personality, manner, and analytic method are more suitable for some cases than for others. And there is no reason to suppose that as analysis develops, a considerable degree of specialisation will not occur. Already most analysts are ready to admit in private that they are better with some types of case than with others.

* See footnote to a paper by Freud on " Female Sexuality," *I.J.P-A.*, 1932, vol. XIII, p. 285 :—" It is to be anticipated that not only male analysts with feminist sympathies, but our women analysts also, will disagree with what I have said here. They will hardly fail to object that such notions have their origin in the man's ' masculinity complex,' and are meant to justify theoretically his innate propensity to disparage and suppress women. But this sort of psycho-analytic argument reminds us here, as it so often does, of Dostojewski's famous ' stick with two ends.' The opponents of those who reason thus will for their part think it quite comprehensible that members of the female sex should refuse to accept a notion that appears to gainsay their eagerly coveted equality with men. The polemical use of analysis obviously leads to no decision."

CHAPTER IX

TERMINATION

1. CRITERIA FOR TERMINATION (Q 6 (1)). *What are your criteria? (a) Symptomatic, (b) psycho-sexual, (c) social? Are your criteria mostly intuitive?*

It is significant that one-third of the contributors failed to answer this question. All who replied stated that they use all three, symptomatic, psycho-sexual, and social criteria. But a majority appeared to think most highly of symptomatic criteria (partly on account of their practical importance). A few, however, pay special attention to psycho-sexual criteria and consider these more reliable than symptomatic. Those who stress the value of social criteria insist that these should include capacity for positive gratification as well as freedom from inhibition. One analyst takes care to " test the weak places " before the end. Another stressed the development of adequate self-feeling, i.e. friendliness towards the self, coupled with capacity for enduring frustration.

A majority admitted that their criteria are essentially intuitive. Their summated experience of the patient gives rise to a feeling, or impression, that the " end " is approaching. A few, however, emphasise that they always test these intuitions intelligently as thoroughly as they are able.

In this section, as in the section on counter-transference, the replies evidenced very considerable diffidence. This was manifested, not simply by the high percentage of contributors omitting to reply, but by a qualifying attitude on the subject of intuition. It would almost appear as if the use of systematic criteria

were a source of guilt, as if only intuitive criteria were free from suspicion. This re-introduces the bugbear of unconscious and pre-conscious assessments of, and reactions to, the patient. Intuitions spring from both *Ucs* and *Pcs* systems of the mind. Unconscious intuitions in analysis no doubt vary as does the capacity to arrive at pre-conscious intuitions. Analysts unaccustomed to marshalling their observations in logical perspective probably find any effort to do so consciously irksome, and so may be inclined to suspect it of being unorthodox and artificial. There is no objection to intuitions (" hunches ") provided they are corrected to a possible 50 per cent. error; nor is there any objection to the conscious correlation of data provided it does not cover some unconscious " blind-spot." Subjective factors must inevitably determine whether individual analysts tend to place greater reliance on intuition or intellectual assessment. The important thing is that they should learn by experience the extent to which they can trust their preferred method and devise suitable checks on their conclusions.

2. TRIAL TRIPS (S.Q. G(4)). *Do you frequently recommend a " trial trip " ?*

Two-thirds of the answers were on the whole against " trial trips " and one-third in favour. One analyst always makes the first two or three weeks probational, while another only recommends probation as a last resort.

3. DISCUSSION OF LENGTH WITH PATIENT (S.Q. G(3)). *Do you discuss the possible length of analysis during the first interview (transferred cases only) ?*

Opinions seemed to be fairly equally divided between such answers as " Yes, stress long time required," and " No, not if I can avoid it," or " Only if asked by patient."

4. LENGTH OF ANALYSIS (Q.6 (2)). *How long do you think an analysis ought to last? Have you an average period for all cases? In this matter do you differentiate between (a) anxiety, (b) obsessional, (c) characterological, (d) psychotic, (e) normal cases?*

Two-thirds only answered this question. Most preferred not to be specific about duration. The average of the few figures given is about three-and-a-half years. One said she "hopes for two years." Five years is the maximum mentioned, though it is known that seven years has occurred. One holds that time factors should be definitely excluded. Another, after years of experience, has never yet analysed a case to a finish but hopes to do so some day.

There were even fewer answers to the question on differentiation, but these tended to follow a clinical grouping, i.e. character cases take longest, then psychotics, then obsessionals, while anxiety cases are ordinarily quicker than any of these. There were some striking individual exceptions to this grouping. Thus one analyst finds obsessionals take longest, and another, anxiety cases (true anxiety cases, not larval psychotics). One replied that he could make no such differentiation. Another that it is much to be desired, but we are still too ignorant to make it.

Here again there is a good deal of diffidence and uncertainty. The answers are cautious and to some extent play for safety. No doubt questions of orthodoxy are partly responsible for this. For some time past the idea of comparatively short analyses (eighteen months to two years) has been rather suspect. And the three-and-a-half years average is probably more a "safe estimate" than an actual average. This is borne out by observations made in clinic practice. The statistics published by Dr. Ernest Jones in the Decennial Report (May 1926–May 1936) of the London Clinic of Psycho-analysis show that 54 patients received

treatment for approximately one year, 23 for two years, 10 for three years, and only 4 for four years. Amongst cases rated as "hysteria" (anxiety, conversion, or character types) 12 gave good results with one-and-a-half to two years treatment and one with four years, while another 12 gave somewhat less satisfactory results in a little over a year. These numbers are admittedly too few to serve as a basis for rigid conclusions but they do indicate that certain types of hysterical disorder can be satisfactorily treated in one-and-a-half to two years.* On the other hand, in the group of obsessional neuroses (17 in all), only 3 gave first-class results, the average length of treatment being between three-and-a-half and four years, in one instance well over four years. This tends to confirm the general impression that obsessional neurotics are more refractory to treatment than hysterics and also suggests that the average optimum length of treatment probably does vary with clinical types which is, after all, a highly reasonable assumption.

Complementary to the adverse opinions on very short analyses, there exists a reluctance to admit very long ones which is possibly due to a factor of guilt. Analysts no doubt feel that lengthy analyses reflect on their technique. There is an element of truth on both sides. From the point of view of etiology it is almost certain that variations in the length of analysis are justified. It would seem reasonable to suppose that easy cases should not take as long as difficult ones and

* It is to be noted, however, that Jones excluded from his statistical report all cases receiving less than 100 sessions, that is to say, up to five months treatment. And this excludes consideration of the possibility of permanent analytic improvement of anxiety cases within four to five months.

that late fixations are more quickly reducible than early ones unless, of course, one takes the view that no symptom or fixation can really be dealt with effectively until the whole mental structure has been rendered plastic. Sometimes that appears to be the case.* An apparently simple phobia (mono-symptomatic and mono-phasic) may prove to be extremely refractory and may not resolve until after some years of analysis. This does not necessarily imply that the whole psyche must be altered before improvement takes place, though it does imply that monosymptomatic constructions are usually " covers " and that deeper psychopathic *foci* behind the symptom must first be removed. But phobias of this kind are exceptional. The common types show quick preliminary improvement (3–6 months), then regress slightly for a further 6 months and clear up slowly and gradually after this, with occasionally a few symptomatic flings when it is suggested that the analysis should terminate. In such instances it seems extreme (almost an obsessional counsel of perfection) to insist that the whole mental structure must be overhauled. It should not be forgotten that psychoanalysis, although the mother science of the new psychology, is clinically a branch of medical therapeutics. Lay analysts, in particular, would do well to familiarise themselves with the professional perspectives which govern not only diagnosis but therapy in general medicine. In the first place, the organic physician neither feels disposed to nor is called on to

* On this point the clinic statistics are not of much help. The limit of four years is frequently overstepped in private practice particularly in the treatment of chronic cases. Periods of seven-year analysis are not uncommon and in outstanding cases periods of ten years or over are not unknown.

apologise for any length of treatment that he may deem necessary. And he embarks on such treatment without any preliminary guarantees to patients or their friends that he will bring about cures.

The whole subject requires much more investigation. Clearer assessment of the optimum length of treatment in different types of case is desirable. This is particularly true of the psychoses and hysterias respectively. It would appear that some psychotic types, if they do improve, may do so more rapidly than some hysterics. The exact significance of age factors has not been determined. And the whole technique of " following up " cases has not been decided. Many analysts appear not to trouble about after-histories and, in any case, there are as yet no exact criteria by which one can assess the lasting nature of results. Unless one assumes that an individual once analysed ought never at any time subsequently to have a mental breakdown, it is necessary to establish standards of cure applicable to the conditions under which each case was originally analysed.

5. SETTING A TERM (Q.6 (3)). *Do you set a terminal period. If so, does it vary with each case?*

If you set a period do you stick to this decision? Do you favour discontinuous analysis as a terminal device?

Do you favour discontinuous analysis as a general policy, e.g. *after the reduction of superficial symptom constructions?*

One-third sometimes set a terminal period. A few set provisional periods. Others set periods by agreement with the patient. There appeared to be a general tendency not to adhere strictly to periods when set.

One-third favour discontinuous analysis, one very definitely, The majority are against it whether as a terminal device or as a general policy.

Two distinct issues are involved here. The more general problem of " setting a term " was dragged out of its original context by the controversy over " active " devices. Setting a period to analysis and keeping to it were suggested by Ferenczi as procedures calculated to force unconscious material into consciousness. In view of the general feeling against " active " devices (see p. 92) it may be assumed that this " forcing " aspect of term-setting is no longer important.* But the policy of setting a term as such still requires discussion. Glover has pointed out that the analyst who takes the responsibility of beginning a patient's analysis should take the responsibility of saying when it should stop. It is clear that a number of circumstances arising in the average analytic practice may permit some evasion of this issue (arbitrary limitations of time, opportunity, money, etc.). Moreover, just as some patients by one means or another see to it that their analysis comes to an end (usually when they have lost or are in process of losing their primary symptoms), so it is possible for the analyst to leave it to the patient to terminate the analysis at any time after the symptoms originally complained of have disintegrated. Incidentally, it would be interesting to know how many analysts bring indirect pressure on the patient to stop, e.g. by raising the issue on various occasions. However this may be, it seems likely that the real cause of uncertainty as to

* Freud has found " setting a term " the only legitimate method of shortening analysis (" Analysis Terminable and Interminable," *I.J.P-A.*, 1937, vol. XVIII, p. 373).

setting a term lies in doubt about the *criteria* of cure. Lack of sound judgment on these matters is almost certain to lead to mistakes in setting a term.

In Discussion (October 18, 1933) of these results, it was suggested that an analysis can only be said to be complete in proportion as understanding of the patient is complete. A character trait, for instance, must be traced to the primary infantile situation in relation to which it developed. Another speaker considered the patient's general attitude a more useful criterion than symptoms; symptoms sometimes disappear after the conclusion of analysis. Another test is the ease and speed with which anxiety is overcome when it is aroused. One speaker issued a warning against " personal " criteria, i.e. judging the patient by the analyst's predilection for any particular outcome, e.g. complete attainment of the genital stage of development. The only important thing is that patients should be enabled to react to their early situations differently, i.e. cope with them better, but not necessarily in a way that the analyst privately considers most satisfactory. One analyst feels content to leave off in proportion as the early situations have been repeated during analysis and their massive anxieties worked through. Reference was also made to the dream criteria mentioned by Abraham.

6. SYMPOSIUM: CRITERIA OF SUCCESS IN TREATMENT. On March 3, 1936, the British Society held a Symposium on " Criteria of Success in Treatment," opened by Dr. Ernest Jones, Miss Ella Sharpe, Dr. Brierley, and Dr. Glover. Summaries of their papers follow.

Dr. Jones began by drawing a distinction between " therapeutic " and " analytic " results, a distinction that " approximates to the comparison between the patient's estimate of the success achieved and the analyst's." " On the purely therapeutic side I would attach the main importance to the patient's subjective sense of strength, confidence, and well-being. By well-being I mean, of course, the potential capacity for enjoyment and happiness, since the actual amount of happiness obtainable will not depend on internal factors only. This

subjective sense signifies that more energy is at the disposal of the ego than was previously, while there is correspondingly less cathexis of id and super-ego 'positions' independent of ego-control. The ego has expanded at the expense of the id and super-ego. The energy of the id is discharged towards the outer world *via* the ego and not independently of it."

". . . one may be misled by mere external activity and capacity. Is this sheer achievement alone or, on the other hand, an expression of a freely functioning personality which is pouring positive feeling into the activities in question?"

"The free flow of positive feeling through the ego is the counterpart of the diminution of anxiety . . . previously unconscious affects are now allowed to flow into consciousness instead of provoking anxiety. As we well know, the most accurate test of this is the patient's reaction in the presence of suitable frustrations or abstinences. Mental health and the capacity for continence go together."

"If one cannot love then life loses most of its meaning. Here again we have to distinguish between apparent and real capacity. If the patient has developed this potential capacity he is likely to be free and in control of himself in other respects also. It means, further, that he must have dealt satisfactorily with the evil and aggressive sides of his nature."

". . . removing symptoms is far from being the best test of therapeutic success. . . . What really matters is not so much the presence or absence of symptoms as the nature of them when present and the amount of psychical significance attaching to them . . . symptoms—at least those of minor import—are often harmless outlets, almost safety-valves. Symptoms which divert large quantities of energy or bind severe anxiety are grave and failure to remove them is serious." Thus, with symptoms "where the feature of dependence is prominent, such as drug manias or alcoholism . . . so long as anything at all is left of the original propensity . . . the tendency to relapse . . . is to be seriously reckoned with."

" . . the main symptom for which the patient originally consulted the analyst . . . has mostly been the last symptom

to disappear." It is a " test case of the fight between the resistance and the analysis."

What are the most trustworthy criteria of analytical success ? One of undeniable weight laid down by Freud was that the amnesia of the third and fourth years should be removed and the events of most traumatic importance in those years be brought into relation with the subsequent neurosis. . . . " But analytical success goes beyond the pathological field altogether. It betokens an understanding . . . of the developmental lines of all the subject's main interests in life . . . so that ultimately one can see his whole life as a gradual unfolding of a relatively few primary sources of interest,"

" Analytical success betokens the highest degree of the favourable results I described . . . when speaking of the therapeutic criteria . . . how then can one hesitate from postulating the quite simple ideal of complete analysis in every case ? The analyst's wishes are not the only factors. Other factors are the patient's strength of motive, his ego-capacity, the quality and adequacy of his defences, together with a number of reality considerations. In general, one aims at carrying the analysis as far as one can. . . . There are, however, cases where a quite satisfactory working capacity has been achieved, where the patient is adequately adjusted to what remains to him of life, and where the attempt to disturb and modify the deepest defences would be tantamount to a severe cost for the sake of a somewhat doubtful idealism. . . . Analytic work should teach us to value balanced judgment above any fanaticism."

Miss Sharpe confined herself to criteria of cure in adults with " specifically untoward " early environment. No isolated criterion of normality can be made the objective of analysis, e.g. the ideal of full sexual potency is attainable in a percentage of cases only. The criteria of cure have to be made in terms of the psychic plasticity which is actually possible for each case. Again, to make the goal of analytic endeavour the analysis of a stronghold against depression, in cases which are not clinically manic-depressive " may be such an ' ideal.' In the cases to which I specifically refer the analysis reveals situations in

which the patient reacts not alone in phantasy terms nor as the incorporated ideal, good or bad, dictates, but according to specific environmental situations which were real enough and which evoked and reinforced primitive phantasy life. . . . The psyche has known no other possibility than the response that it has made to a specific environment and specific people. Our task is to make a more propitious outcome possible, if we can, in a transference environment that is not repeating the past." One cannot hope to obliterate the past as if it had never been. But the transference provides the chance of dramatising the old situations and therefore of loosening the hitherto stereotyped methods of dealing with all impulses originally associated with them. The attainment of psychic plasticity attainable may, however, be limited, e.g. " a born musician can be nothing else." Criteria of success may be enumerated as follows (due to expansion of ego and mitigation of super-ego severity) :

(*a*) Doing something seriously instead of playing at it. " This may entail the removal of actual psycho-physical inhibitions such as vision or hearing defects."

(*b*) Ability to carry through and complete work. Also to work with more pleasure and less irrational sense of duty.

(*c*) Modification of guilt regarding homosexuality will result in more stabilised friendship supplying a channel for libidinal satisfaction.

(*d*) Increase of social contact and interests.

(*e*) Socialised discharge of aggression, mental, and physical.

(*f*) Capacity for enjoying the society of the other sex, whether full heterosexuality attained or not.

(*g*) Self-understanding gained through analysis should suffice to carry the patient through subsequent psychic stresses. There is no " cure once for all " but " danger signals and the course to take can be a matter of knowledge."

Dr. Brierley limited herself to two points, the comparative reliability of criteria and the relativity of success. The process of cure is essentially dynamic. Success is the result of the mobilisation and re-distribution of energies in the psyche and the vital factor in this is the freeing of affects and the

resolution of anxieties. . . . Ego-expansion and improved reality adaptation, that is, a relatively greater discharge of energy through the ego coupled with increased tolerance of frustration and diminution of super-ego and symptom charges are more important than the total disappearance of symptoms. The amount of re-living in transference is as important as the amount remembered.

Psycho-sexual are more important than symptom criteria, because (*a*) erotic gratification whether direct or sublimated makes life most worth living, and (*b*) adequate freeing of erotism implies adequate solution of aggressive problems. The general balance in the psyche is more significant than the ability to procure complete sensual gratification. The adaptation value of sensual potency varies with the individual.

The essential " economic " changes in the psyche have to be inferred from change in mental attitude, feeling, and behaviour. The ease and rapidity with which emotional crises can be surmounted is a better test than their non-occurrence. There is no foot-rule which can be applied to all cases alike. It is a matter of estimating whether enough has been done to permit satisfactory continued development on individual lines. In assessing " economic " change all the recognised criteria may be employed. In considering the ground covered in analysis the connection of phantasies with infantile reality is important. Further, if the major infantile affects have been transferred to the analyst and have been adequately analysed, the patient should become willing to leave off. Conclusion by mutual consent has the best prognosis. A recapitulation of anxieties following a provisional agreement to stop, say, in three months, is favourable. It represents the patient's own " checking-up."

Sometimes both internal and external factors limit the degree of therapeutic success which can be hoped for. In some cases partial analysis may give better practical results than theoretically more complete treatment. Patients who get some gratification out of the process of analysis, as distinct from the results achieved by it, usually do well. The kind of gratification varies, but where the process is a suitable outlet for some

strong urge, this modicum of satisfaction seems to off-set the inevitable frustrations in a helpful way.

Dr. Glover said :

A. That a fairly thorough way of estimating success was to run through the different factors concerned. Thus one might start with instinct and enquire whether self-preservation functioned adequately or was inhibited. This referred to working capacity and the capacity to refrain from self-injury. Whether love manifested itself in erotism, affection, or through social channels, and whether it was inhibited or inverted. In the same way, whether aggression functioned, either directly, in hating, or indirectly, by making the patient hated, or whether it was inhibited or manifested through internal inhibition. Also whether hating capacity was out of proportion to the reality stimulus or whether it was unduly inhibited in view of the reality stimulus. Consideration of reality adaptation involved both symptom and super-ego assessment.

B. Special clinical factors offered another method of assessment. Thus, alterations in the content of and affective reaction occurring in dreams were extremely serviceable criteria. Stereotyped anxiety and terror dreams often showed not only a reduction in the amount of symbolic expression during analysis but a material reduction in the quantity of anxiety. Contrariwise, the persistence of such dreams was an indication that analysis was not complete even if the symptoms had disappeared. Similarly, a reduction in the amount of exploitation of any one set of mental mechanisms, e.g. flight, denial, obsessional reaction formations, projections, etc., was a favourable indication. Emotional and affective reactions of an exaggerated type should be reduced to a reasonable level. Absence of modification contra-indicated termination. Capacity to adapt to special strains, deaths in the family, childbirth, loss of money, physical illness, etc., should be taken into account.

C. Significant clinical details, e.g. the manner of shaking hands, eating and clothing habits, behaviour in regard to money, capacity to " play," all offered " test " material.

In conclusion, the problem of analytic criteria is difficult

to render " scientific," but it offers ample scope for obsessional elaboration. Complete analysis would mean complete analysis of all phantasy and defence systems. But all standards of completeness are subject to two considerations.

1. Irrespective of variations in theory, technique, and standards, the average results of all analysts appear to work out about the same.* As to the time factor, the longest cases often have the worst results symptomatically, but sometimes are conspicuously successful in other directions (character changes).

2. Subjective factors in the analyst influence the individual choice of criteria. The greater the analyst's uncertainty the greater in all probability his obsessional compulsion to concern himself with ideal standards. In the same way, the less exact the information, the more oracular or religious the pronouncement. The biggest margin of error occurs where preconceived analytic standards are rigid. . . . Since we are first and last clinical psychologists we should be well-advised to base our estimates of success first of all on clinical evidence and not on any theoretical preconceptions.

It is interesting to compare the results of the Questionnaire on Termination with those obtained in the Symposium on Criteria of Success. As has been said, the greatest diffidence was manifested in the replies to direct questions. This was much less obvious in the symposium, but there was in the latter case a considerable blending of theoretical with practical criteria. What is really urgently needed is a detailed list of the practical indications for terminating an analysis, a list which will allow for variations in the clinical type of case. Admittedly, this can be compiled

* This conclusion has been disputed on various occasions. The statement is based on observations made on the results of analyses in clinic practice, the results of training analyses, and on the results observed with cases distributed to various analysts in private practice.

only when there is fairly broad agreement on the general principles, a state of affairs which has not so far been reached. Obstacles to this general agreement vary in type. No doubt the cruder factors could be eliminated, e.g. the influence of the state of the analyst's practice on pre-conscious assessments. There is an obvious temptation for the busy analyst to wish for a rapid termination of each case: there is an equally obvious temptation for the less busy analyst to be a little more " thorough " than the occasion demands. But these obstacles should be easily dealt with under the heading of " economic counter-transferences." More difficult to cope with are the " scientific counter-transferences "; in other words, the preconceptions as to length and thoroughness of analysis which are due to bias in etiological views. And these pre-conceptions can be influenced very profoundly by idealisations of the " complete " analysis. These are frequently covers for perfectionist systems of a " guilt type." It would help to correct all such vitiating factors if all analysts could secure greater clinical experience. Consulting practice in analytical circles is not extensive and lay-analysts are debarred by professional etiquette from undertaking this type of work. Many analysts treat only those cases which are transferred to them for treatment and have little or no idea of clinical standards. Moreover, " simple " cases are the special preserve of commencing analysts. More experienced practitioners usually find that in course of time their practice consists mostly of difficult cases that have gone from pillar to post. This accounts in part for the increasing length of analyses. Perfectionist standards make it difficult for the analyst to

admit that his analysis of a particular case may fall short of his own criteria for termination. This encourages him to subscribe to a clinical fallacy, namely, that all cases are equally amenable to analysis. Obviously the fact that on the whole analysts get the same therapeutic results in any one case-group does not imply that cases taken from different groups are equally curable. The belief that all cases are curable is essentially obsessional if not indeed rather grandiose. No analyst need be ashamed to admit that he has gone as far with any case as is possible. This admission would save him a good deal of self-questioning and his patient, on occasion, a good deal of time.

CHAPTER X

PSYCHOTIC CASES

1. DIFFERENCES IN TECHNIQUE (S.Q. F(1)). *Does your technique in such cases differ materially from your customary procedure?*

Five analysts only replied that their technique differs materially. Five others modify their technique to some extent, e.g. "interpret cautiously," "more passive," one "interpret more directly." Five more answered "no," or "not materially," while a few replied that they had no experience to go on.

2. REASSURANCE (S.Q. F(2)). *Do you reassure more?* (*See* S.Q. B(1 and 2), p. 44).

A majority replied "yes," one specifying "with melancholics, not with manics."

3. FRIENDLINESS (S.Q. F(3)). *Are you more friendly?*

Answers fairly equally divided between "yes," "no," "not necessarily." One said "may be suspected by patient."

4. SUPER-EGO ROLES (S.Q. F(4)). *Do you play super-ego roles?*

There were many gaps in these answers, but a majority of those who did reply said "no." Only one gave an unequivocal "yes." Other replies were "not as a rule," "seldom," "give advice," "only protective."

Discussion on May 2, 1934, resolved itself into a consideration of the advantages and disadvantages of reassuring psychotic patients, whether in crises or not. Thus it was maintained

by one speaker that since progress is obtained by the patient introjecting the analyst as a good object, he should in fact be a good object. Objection to this took two directions: (*a*) that reassurance has no effect on psychotics, and (*b*) that it may dangerously increase anxiety if the patient feels that the analyst is in his power. Another speaker said that it was all-important that the analyst should not himself be afraid of the patient's aggression and that in psychotics it is specially necessary to dose anxiety. This can be done by a combination of the right type of reassurance, by interpretation, and sometimes by suitable alteration in the environment.

If the answers on technique in psychotic cases appear extremely scrappy and unilluminating a good deal of the responsibility must lie with the uninspired form of the questions. The only satisfactory way of checking information on this subject is to enumerate typical situations arising during the analysis of a number of different types of psychosis, e.g. " How do you handle the suicidal threats or crises in hysterical and melancholic cases respectively ? " or " How do you deal with the anti-social outbursts of schizoids or delusional alcoholics ? " The form of the questions circulated was determined by a number of enquiries made on the more general point, viz. " Does analytic technique differ in psychotic cases ? " One fairly accurate conclusion may be drawn from the form of this enquiry as well as from the general nature of the replies, viz. that experience with typical psychotic cases is the exception rather than the rule. Analysts have abundant opportunity of studying the effects of, for example, the exaggerated action of projection mechanisms. They can observe these effects in the course of analysing psycho-neurotics, or they may

have more frequent opportunities to do so when analysing character cases. And, particularly during hysterical crises, they can study at first hand some major disturbance of reality sense. But all this is not the same thing as analysing true psychotic cases. And it is precisely because psychotic cases may suddenly react violently in unexpected directions that the problem of modifying conventional analytical technique is so important. In the analysis of a psycho-neurotic case, crises are seldom of so dramatic a nature, and it is obvious from the replies on this subject (see Section II, " Anxiety," p. 36) that most analysts hope to be able to deal with these by routine interpretation. Psychotic cases have not only more severe crises but show a number of exacerbations any one of which is more difficult to cope with than a psycho-neurotic crisis. These are part of the symptom picture. The same is true of crises occurring in cases of alcoholism, drug addiction, acute perversions, schizoid delinquents, etc.

In the earlier phases of treatment these may not (sometimes cannot) be avoided by any of the procedures employed in dealing with sporadic anxiety in the psycho-neuroses. When real psychotic crises do occur the first and most obvious discovery made by the analyst is that the ordinary guiding rules fail him and that he must deal with the problem to the best of his individual ability on his own initiative. At this juncture a sound training in clinical psychiatry will stand him in good stead. In particular, it will prevent his being handicapped by undue anxiety, fear, or guilt, and it will aid him in deciding the best policy to pursue. Typical problems are, for example, the indications for isolation, narcotisation, temporary restraint or, in the

case of addiction, the advisability of forced abstinence, etc. In lesser crises it is often necessary to make decisions as to the nature of analytic contact (when, where, and how) and the nature and amount of family and social contacts, work or other extrinsic factors. On such occasions technique is altered simply because the analyst is left no alternative. Apart from handling crises, the most difficult problems of technique in psychoses arise with genuinely " ambulant " types whose crises are more amenable to ordinary technique, and it is important to determine what alteration in customary procedure is desirable. Unfortunately no exact indications were given by contributors as to the nature of such alterations. Differences of opinion as to the influence of reassurance in psycho-neurotic crises hampered the contributors not a little. Definitions of the principles involved were conspicuous by their absence. What is really needed is a careful examination, not of the resemblances between psycho-neurotics and psychotics, but of their essential differences. To take one example : the general routine of analysis, in particular, the " fringe contacts " between analyst and patient, have much deeper significance for the psychotic than for the neurotic. No doubt the symbolic meaning of such contacts is the same for both groups, but the real reaction is different. The psychotic reaction to time, place, and detailed circumstances, including weather, time of day, and so forth, is much more profound than is the reaction of the psycho-neurotic. The slights and hurts re-experienced by depressives in the transference are much more painful, the paranoid reactions to noise, eavesdropping, smoke, etc., more acute. Although the interpretation

of such details is, generally speaking, the same in both groups, the estimation of their significance is different. Hence transference interpretation should pay much more attention to the symbolic speech of the psychotic as compared to the non-psychotic patients and less to the more ostentatious, relatively complex and superficial transference phantasies. Only by comparison, of course; in some depressive cases even superficial transference phantasies can obstruct the course of the analysis just as effectively as in acute hysteria. No doubt this method of approach might be applied in a systematic way to all the characteristic peculiarities of psychotic function. In particular, a careful comparison should be made of the spontaneous curative efforts of neurotic and psychotic subjects respectively. If concrete technical suggestions could be made on the strength of such investigations, they could no doubt be modified and employed with advantage in the treatment of drug addiction and the more archaic perversions. These latter frequently correspond, in the psycho-sexual sphere, to psychotic changes in ego structure. Obviously super-ego aspects require careful investigation along the same lines. This will involve frequent Questionnaires. The results given here may serve to indicate the comparative uselessness of certain types of question.*

* For later work on technique in the psychoses see Appendix III, p. 159.

CHAPTER XI

Relation of Theory to Practice

In psycho-analysis, as in other sciences, there are two more or less conventional and opposing attitudes towards the interrelation of theory and practice. There are those who hold that a good theoretical understanding is essential to good practice. Theory, they maintain, is a generalisation from the " particulars " of clinical experience. And providing they are subjected to correction and modification as clinical knowledge expands, most theoretical generalisations can function as valuable aids to research. Thus the practitioner who is familiar with theory has the advantage of seeing the wood as well as the trees. He may also perceive new woods, hitherto unobserved. On the other hand, the clinician pure and simple may retort that, if the observer has a " favourite " theory, however accurate this may be, his observations will tend to be moulded to this theory. He will continue to see the same wood in every collection of trees. If he does make fresh discoveries he will be liable to depreciate them by insisting that these apparently new woods are the old ones over again. In short, the pure clinician will maintain that theoretical orientation is a fertile source of mistakes due to " bias." The conventional compromise usually arrived at is that theory and practice have a useful reciprocal relation

but that both approaches must be carefully checked for possible errors.

In psycho-analysis, the question is more complicated. A predominantly theoretical interest has always been rather suspect. Indeed, many young analysts are quite frankly afraid to admit an interest in theory lest it should be construed as a sign of lack of conviction. The suspicion is based partly on intuition and partly on clinical observation. It has been noted that resistances during treatment can take the form of friendly or hostile intellectual preoccupation with analytic views. Obsessional cases and certain schizophrenic types show a genius for intellectual elaboration of a somewhat unrealistic kind. This may and sometimes does help on the analysis, but more commonly it functions as a powerful unconscious defence-mechanism. It would be interesting to find out whether the reactions of analysts to the relations of theory and practice could be classified in terms of their own psychological constitution, e.g. their affective disposition, type of phantasy thinking (concrete and corporeal or mainly abstract), degree of libidinisation of thought, and so forth. It would, no doubt, be possible to distinguish, for instance, between " anxiety " types and " obsessional " types (not, by the way, that this would exhaust the catalogue of reaction-types),* and so lay down a broad distinction between analysts for whom the realm of hypothesis would be an emotional battle-ground and those for whom it would be a comparatively safe play-room. Moreover, original thinkers, like other creative artists, may feel a concern

* Edward Glover, " Introduction to the Study of Psycho-analytical Theory," *I.J.P-A.*, 1930, vol. XI p. 472.

for the perfection, and hence for the preservation of their " good " theories. And they may in consequence develop a corresponding distaste for scientific or critical evaluation of them, a sensitiveness which might well be concealed from themselves owing to a righteous feeling of preserving the " good." On the other hand, their critics may have motives ranging from an extreme of protective to an extreme of destructive urge. Now there is some evidence for the view, that objectivity in thinking is not incompatible with a fairly high degree of ambivalence provided the latter is accompanied by a certain elasticity in the use of introjective and projective mechanisms. The hostility provides a certain " detachment " in which fair or constructive assessment can be made. But it is manifestly impossible to conduct an investigation of this intimate sort by means of a Questionnaire, quite apart from the fact that, in such circumstances, the conscious judgment of the contributors might be under suspicion of rationalisation. The questions actually sent out were intended to focus attention on the more practical consequences of theoretical bias. One preliminary query was inserted in order to bring out any general preference for either of the conventional views and was followed by specific test questions.

1. **PRECONCEIVED THEORY AND CASE APPROACH** (Q.10 (1)). *Do you approach each case with a preconceived theoretical outline of development to which you expect the case to conform? Or do you approach each case with a preconceived outline modified by your knowledge and valuation of clinical features (diagnosis or prognostic criteria)?*

Several analysts felt unable to reply, or replied only in part to this question. The rest appeared to be fairly equally divided. Thus, one-half denied that they approach analysis with a preconceived theoretical outline in mind. Half the remainder admit to a very general outline, partly theoretical, partly modified by diagnosis and prognostic expectations. A few admit outright to a " modified " outline, and two more admit such an outline but try to minimise its importance. Thus one wrote of " unwelcome realisation of expectation of conformity " on the part of the patient. Feelings of guilt about the existence of a preconceived outline were prevalent. Some contradicted themselves; they denied any outline, but later went on to specify a standard etiology for all cases or to accentuate the pathogenicity of aggression alone. One commented on the general relation of theory to practice : (*a*) that it is impossible to approach any analysis without preconceptions ; (*b*) that it is less obvious where cases are passed on after consultation by someone else, but more obvious where the analyst has functioned also as consultant ; (*c*) that it is desirable to have preconceptions provided these are based on accurate information, but that (*d*) analysts are hampered by the absence of sufficient etiological information and are therefore inclined to play for safety.

These answers were much more open than might have been hoped. The differences of opinion that emerge seem to indicate a genuine uncertainty as to the ideal combination of theoretical and clinical interest. The most striking replies were those in which the contributor's general attitude was at variance with his clinical practice.

2. RELATIVE PATHOGENICITY OF INSTINCTUAL FACTORS (Q.10 (2)). *Do you have preference in regard to the pathogenicity of instinctual elements,* e.g. *value the factor of aggression more than the factor of primitive sexuality ?*

A majority find the combination of sexual and aggressive factors pathogenic. These constitute an " open " etiological school. A small " closed " school minority is about equally divided into supporters of sexuality alone and aggression alone. A few say they have not made up their minds on this problem. There is fairly general agreement among members of both schools as to the therapeutic importance of analysing aggression.

3. ETIOLOGICAL RATING OF ŒDIPUS COMPLEX (Q.10 (3)). *Do you regard the genital (phallic) œdipus complex (positive or inverted) as the main factor (a) in neuroso-genesis, (b) in the genesis of psychoses, (c) in characterological cases ?*

A majority favour the cardinal importance of the œdipus complex in neuroso-genesis. [This is a " closed " etiology and in some instances the view contradicted an earlier denial of having any preconceived outline (see Q.10 (1)).] Two strongly emphasise the importance of pre-genital factors in *all* cases. There is much less certainty as to the rôle of the œdipus complex in psychotic and character cases, with some bias towards conceding a relatively greater importance to pre-genital factors in such cases.

4. PRE-GENITAL PHANTASY SYSTEMS (Q.10 (4)) *Do you regard pre-genital phantasy systems as being secondary (regressive, defensive) products relating to genital œdipus traumata ?*

Rather less than half the answers said pre-genital phantasies were wholly or mainly secondary. Rather more than half said they have " primary " importance but may be regressively " reinforced."

5. CLASSIFICATION OF INFANTILE ANXIETIES (Q.10 (5)). *Do you regard castration anxiety (a) as the only anxiety factor of significance in treatment, (b) as one of a number of anxiety situations, or (c) as a cover for pre-genital anxieties ?*

A majority regard castration as *one* of a number of important anxieties. This again appears to contradict the earlier majority in favour of the cardinal importance of the œdipus complex in neuroso-genesis ((3) above). Two, however, definitely specify that castration anxiety is the most important in neuroses. Only one still regards it as the only significant anxiety. A minority remarked that castration anxiety may serve as a cover for earlier anxieties. There appeared to be some confusion between the possibility of recovering earlier anxieties through analysis (e.g. one said all anxiety could be traced to oral anxiety) and the relative importance of specific anxieties in different pathological conditions.

6. ASSESSMENT OF DEFENCE-MECHANISM (Q.10 (6)). *Do you favour the view that mechanisms of projection and introjection are (from the therapeutic point of view) more important than the mechanisms of repression, regression, etc.? If so, do you believe this applies in all cases,* e.g. *in the analysis of a hysteric as well as in the analysis of a psychotic character?*

There were very few answers to this question. Three denied the greater importance of introjection-projection, while two affirmed it for psychotic and character cases. Four believe in the greater therapeutic importance of analysis of introjection-projection phenomena in psychotics. They hold that no therapy is possible in such cases unless this is done, whereas in neurotics, e.g. hysterics, improvement is possible without it.

The practical aspect of this enquiry lies in its bearing on the suggestion that a preference for special pathogenic factors may influence the content of interpretations. Scrutinising the replies from this angle it would appear that the unconscious pathogenic content (to use the simpler, if old-fashioned, phrase) most commonly interpreted is roughly as follows (*a*) in the psycho-neuroses: the classical œdipus situation with its characteristic guilts and anxieties (castration anxiety)

together with a varying quantity of anxiety and guilt associated with pre(phallic) genital organisation. These latter anxieties may have predisposed to the classical œdipus conflict or exacerbated it, or they may have been re-animated to provide a cover for œdipal conflict, (*b*) in psychotic or character cases; the guilts and anxieties of early pregenital organisation (particularly oral manifestations ?) together with a varying degree of ventilation of the classical œdipus situations. Expressed roughly in terms of ego and mechanisms the formula would run (*a*) in the psycho-neuroses: analysis of the classical super-ego organisation (including the mechanisms of introjection, projection, etc.), that contribute to this organisation together with analysis of the products of faulty (defensive) repression, displacement, etc.; (*b*) in psychoses and character cases: exhaustive analysis of the projection-introjection phases of primitive ego–super-ego organisation together with adequate analysis of the later ego and its exploitation of repression, etc. If this review be accurate it may be assumed that analytical etiology is anyhow capable of division into two main varieties which correspond with two main clinical groups in a rough classification of psycho-pathological states.

This is where the difficulties really arise. It is probable that the answers in favour of classical etiology were to some extent influenced by timidity or tradition. But it must also be admitted that, although many analysts detect a genuine discrepancy between the infinite variety of psycho-pathological states and the old stereotyped and simple etiological formulæ, to replace these by a plain double-barrelled etiology such as given above is much too simple. If we begin

to differentiate, why not be thorough about it? But at this point guilts and preferences begin to operate anew: guilts, because we may be going too far and possibly weakening unduly the idea of a kernel œdipus conflict; preferences, because we may wish to substitute for the older "closed" etiology a new but equally "closed" system. From the point of view of untrammelled investigation, that would be out of the frying-pan into the fire.

The fact remains that there is fairly general uncertainty as to the advantages of "open" and "closed" etiologies respectively. Despite this fact, it might be maintained that however uncertain the analyst may be in this respect, there is no reason to suppose that he abandons an empirical attitude to each case. It might even be held that uncertainty fosters true empiricism; further, that there is no reason to suppose that interpretations are necessarily biased or falsified by theoretical predilections or doubts. Unfortunately these idealisations do not always tally with the facts, as the following example will show: At a recent meeting of the British Psycho-Analytical Society a specific issue arose during discussion of a clinical case. Referring to the relation of unconscious phantasies to emotional environmental influences, the speaker stated that in his view the patient's mother had had a bad influence on her son's infantile development and hence on the formation of his symptoms. Combining his analytical observations with information gathered from the patient's memories and from indirect sources (including both hearsay and direct observations made by relatives), he concluded that the mother had aroused considerable hate in the child (who had, he believed,

sensed the mother's true character with fair accuracy) and at the same time stimulated his guilt to such an extent that it blocked all direct expression of the hate. The discussion which followed revealed significant differences of opinion as to the correctness of these conclusions. In the first place, it was suggested that, since the analyst's views were mere inferences or estimates of probability, the matter could only be settled by more detailed analysis of unconscious content. Moreover, it was held by some that generalisations on the relations of any one patient to his mother would require to be confirmed by comprehensive investigations into the whole subject of the relations of babies to their mothers ; hence, that the reports on this particular case were not sufficiently unbiased and objective. Some members went further and maintained that not only were hearsay reports and the memories of patients unreliable, but that, as analyses proceed, a change from an originally hostile judgment of the mother to a (presumably more realistic) friendly one can frequently be detected, the implication being that most stories of infantile mother-hatred were really a cover for deeper love attitudes. This view was expanded by one member to the effect that the evidence did not justify the conclusion that this mother was a " bad " mother and that, had interpretation been more accurate, it would have transpired that the mother was not bad but that the child was in conflict over the possible ineffectiveness of its own capacity to love and therefore to preserve and restore her as an internal object. This criticism obscured the issue somewhat, because obviously the two views are not incompatible. It so happened that in this instance

the analyst's conclusions could be put to the test by comparing them with reports from relatives in the possession of the consultant (E. G.) who first saw the case. Examination of these reports on the mother's character from her daughter, her son-in-law, and some of her friends (including one from a friend who had known her from her childhood) proved fairly conclusively that the analyst and the patient were correct in their estimates. The mother had, in fact, had a consistent policy throughout her life of emotionally exploiting dependents, especially her children, and preventing any exhibition of resentment by making them feel guilty. She had a high opinion of herself as a mother but was, in fact, self-aggrandising, tyrannical, and selfish. People who did not know her well were inclined to fall in with her version of herself and to show considerable devotion to her interests. When they became more intimate they developed strong hostility to her emotional steam-rollering. It should be clear that this proof of the existence of a really " bad " mother does not do away with the necessity for exploring the patient's own sadism, nor does it exclude the part played by his own impulses in creating his pathological anxieties. It does suggest, however, that such anxieties are readily reinforced by external factors. It also throws some light on the way in which theory may influence practice, for it suggests in no uncertain manner that, had the case been analysed by one of the more severe critics, the interpretations given would have glossed over the environmental factors and accentuated the endopsychic factors. Such interpretations would, in this instance, have been unbalanced and would have tended to maintain guilt rather than to

ventilate plain hostility. In fact, the analyst would have continued the mother's own policy of playing an intolerant super-ego rôle to the patient's id. This case may or may not be an exceptional one. But at least it does underline the risk that preconceived theoretical views can influence the process of interpretation unfavourably as well as favourably. No doubt this danger is generally agreed. But something more than agreement is necessary. Other pit-falls have to be avoided, in particular the view that opinions with which one does not oneself agree are dangerous and destructive.

APPENDIX I

A. ORIGINAL QUESTIONNAIRE

(issued July 8, 1932) (Q)

1. INTERPRETATION

1. FORM.

Do you prefer :

(1) short compact interpretation, or

(2) longer explanatory interpretation, or

(3) summing up type : (*a*) trying to *convince* by tracing development of a theme ; (*b*) proving (or amplifying) by external illustration.

2. TIMING.

Query : favourite point of interpretation ?

(1) early in session ;

(2) middle or before end (allowing a space for elaboration) ;

(3) at end : " summing-up " fashion.

3. AMOUNT.

(1) General : as a rule do you talk much or little ?

(2) Early stages : how long do you usually let patients run without interference ? How soon do you start systematic interpretation ?

(3) Middle stages : is your interpretation on the whole continuous and systematic, or do you return from time to time to the opening system of letting them run ?

(4) End stages : do you find your interpretative interference becomes incessant ?

4. DEPTH.

(1) (This can be thought of in terms of degree of repression, conscious accessibility, and readiness or in terms of stages, e.g.

pregenital as compared with genital interpretation, etc.) *Please state individual definition.*

(2) Do you have a standard level of deep interpretation for all cases, or do you have an optimum depth varying for clinical conditions, e.g. in (*a*) anxiety, (*b*) obsessional, (*c*) characterological, (*d*) psychotic, (*e*) normal cases.

(3) On the whole, do you favour deep interpretation in early, middle or late stages ?

(4) Do you favour deep interpretation as the ideal criterion or deep interpretations in terms of the reality circumstances (1) infancy, (2) childhood, (3) puberty, (4) adolescence, (5) adult life ?

2. SPECIAL PROBLEMS

The problem of *anxiety :* What is your favourite method of dealing with this ? E.g. by rapid interpretation of (*a*) " repressed " content, or (*b*) " repression " factors, or by slower expansion of the emotional state combined with a degree of reassurance (postponing deep interpretation till later).

3. USE OF TECHNICAL TERMS

(1) *Explanation of mechanisms.* What practice do you favour, e.g. do you talk about introjected objects or organs, or do you use " super-ego " nomenclature ?

(2) *Technical explanations or orientation.* Do you usually act as a source of information (reality) on (*a*) sexual, (*b*) non-sexual subjects ?

4. " FORCED " PHANTASY

(The term " forced " (Ferenczi) is not very satisfactory : the interpretation of an incest phantasy is equally " forced.")

(1) Do you, on the strength of isolated words or actions, without waiting or hoping for confirmation in associations, expound phantasy systems affecting, (*a*) reality relationships, (*b*) transference relationships ?

(2) Do you abandon the association rule in certain instances holding the patient to one thread until you have constructed a phantasy piecemeal ?

(3) Do you ask direct questions (*a*) about matters of fact, e.g. family history, (*b*) about matters of phantasy, (*c*) about emotional reactions ?

5. "ACTIVE" DEVICES

(To avoid lengthy definition, this may be taken in the "Ferenczi" sense.)

Preliminary. Apart from laying down the association rule do you give any general or specific recommendations re suspending personal habits (masturbation, etc.) before analysis commences ?

During analysis. Do you employ prohibitions or *positive injunctions ?*

(1) aimed at symptom habits, phobias, obsessions ;

(2) aimed at psycho-sexual habit, masturbation, perversion, fetich, intercourse (*a*) marital, (*b*) extra-marital, promiscuity.

(3) aimed at social habit. If practice varies, give indications for policy in different cases.

6. TERMINATION OF ANALYSIS

What are your criteria : (*a*) symptomatic, (*b*) psycho-sexual, (*c*) social. Are your criteria mostly intuitive ?

(2) How long do you think an analysis ought to last ? Have you an average period for all cases ? In this matter do you differentiate between (*a*) anxiety, (*b*) obsessional, (*c*) characterological, (*d*) psychotic, (*e*) normal cases ?

(3) Do you set a terminal period : if so, does it vary with each case ? If you set a period do you stick to this decision ? Do you favour discontinuous analysis as a terminal device ? Do you favour discontinuous analysis as a general policy, e.g. after the reduction of superficial symptom constructions?

7. THE NEUTRALITY OF THE ANALYST

(Lay-figure Concept.)

(1) Do you ever admit to the patient the possibility of being wrong ? Do you ever admit to the patient the possibility of "not knowing" ?

(2) Do you lay emphasis on the cooperative nature of analysis, as distinct from the phantastic cooperation demanded in transference ?

(3) Do you believe in giving some indication of a positive friendly attitude as distinct from the friendliness implied in " professional interest " ?

8. FEES

(1) Do you have a standard rule re payment for non-attendance ? Do you keep to it ?

(2) Do you ever raise fees during analysis ? If so, when ?

(3) Do you accept presents from patients ? If so, on what system ?

9. EXTRA-ANALYTICAL CONTACT

(1) Beginning and end of sessions : Do you shake hands before or after ? Do you use small talk ? Do you permit small talk ? Do you lend books, etc. ?

(2) *Extra-mural.* Do you meet your patients socially ? Do you avoid meeting patients socially ? If so, why ?

(3) *Family contact.* Do you interview members of family (*a*) with, or (*b*) without patient's knowledge ?

(4) Do you analyse patients who have emotional ties (friendships, etc.) to your own friends or family, or who are related to or friendly with individuals in any way dependent on you ? On these matters have you any guiding principle ?

10. RELATION OF THEORY TO PRACTICE

(1) Do you approach each case with a preconceived theoretical outline of development to which you expect the case to conform ? Or do you approach each case with a preconceived outline modified by your knowledge and valuation of clinical features (diagnostic or prognostic criteria) ?

(2) Do you have preference in regard to pathogenicity of instinctual elements, e.g. value the factor of aggression more than the factor of primitive sexuality ?

(3) Do you regard the genital (phallic) œdipus complex (positive or inverted) as the main factor (*a*) in neuroso-genesis, (*b*) in the genesis of psychoses, (*c*) in characterological cases.

(4) Do you regard pregenital phantasy systems as being secondary (regressive, defensive) products relating to genital œdipus traumata ?

(5) Do you regard castration anxiety (*a*) as the only anxiety factor of significance in treatment, (*b*) as one of a number of anxiety situations, or (*c*) as a cover for pregenital anxieties ?

(6) Do you favour the view that mechanisms of projection and introjection are (from the therapeutic point of view) more important than the mechanisms of repression, regression, etc. ? If so, do you believe this applies in all cases, e.g. in the analysis of a hysteric as well as in the analysis of a psychotic character ?

11. Please add a list of your special problems in technique and of questions you would like to hear discussed.

B. SUPPLEMENTARY QUESTIONNAIRE

(issued September 30, 1933).

A. A PRELIMINARY PROBLEM OF ROUTINE RAISING MATTERS OF PRINCIPLE

(1) Do you smoke during work ?

(2) Do you permit (or invite) patients to smoke ?

(3) If you have *no* fixed practice, what indications do you follow, e.g. with (*a*) what types of case do you either smoke yourself or permit (invite) them to smoke ; (*b*) what types of situation in any one case ?

(4) If you have a *fixed* practice, on what general principle is it based ?

(5) What do you understand by the term " abstinence " ?

B. ADDITIONAL TO THE PRINCIPLES OF INTERPRETATION

(1) (Since the use of terms such as " deep interpretation " and " reassurance " may introduce some artificial distinctions.) *What do you understand by the term " reassurance " ?*

(2) If you accept the term as distinct from interpretation, do you regard it as an essential part of analysis, or is it only an emergency measure ?

C. PRACTICAL MATTERS ARISING TO SOME EXTENT FROM "B"

(1) Do you always keep strictly to the same length of session ?

(2) Do you allow a gap in your time-table to permit of extending sessions ?

(3) Do you see patients again in the same day ? If so, for long or short periods ?

(4) Do you allow telephone conversations ?

(5) Do you read letters, look at photographs, etc., brought for your inspection ?

(6) Do you keep strictly to the free association rule or permit (advise) relaxations of it ?

(7) Do you communicate personal opinions (e.g. cultural, social, or political views) ?

(8) If you have no fixed or rigid practice in any of these matters, what indications do you follow ? For example, even if you hold that in general the analyst should be a neutral or shadowy figure, are there instances where you believe he should be more of a "real" figure for the patient ? If so, in what respects ?

(9) How far should the spontaneity of the analyst be restricted ? Are there any dangers or drawbacks in such restrictions ?

(10) How far do you think the patient's unconscious capacity to read the analyst's psychological tendencies should be allowed for ? (This involves the problem of how far the transference situation is solely a projection or how far it is a (conscious, unconscious) recognition of the real (conscious, unconscious) attitudes of the analyst ?

(11) Sample problem arising from the foregoing : Would you admit change of mood, anxiety, or personal illness to the patient ?

D. VARIOUS PROBLEMS IN INTERPRETATION

(1) Do you explain the nature of symbols in dream interpretation ?

(2) Do you think " reconstruction " helps or hinders the recovery of memories ?

(3) Do you attach special value to childhood memories ?

(4) In what clinical type do you meet with the maximum of recovered memories ?

(5) What is your most successful line of interpretation dealing with (*a*) excessive loquacity, (6) (*b*) obstructive commonsense, (7) (*c*) patients who need constant encouragement to talk, (8) (*d*) the spontaneous offer of infantile interpretations by the patient ?

(9) Do you allow time for " working through," i.e. refrain from additional (new) interpretations for a time after uncovering highly charged emotional situations ?

(10) Do you open up anxieties from different sources simultaneously, or do you concentrate on one main source at a time ? (The same question in another form below (11)).

(11) Does the stirring up of anxiety from various sources simultaneously increase or diminish the total amount of apprehension (resistance) ?

(12) Do you as a general rule interpret id impulse first, or super-ego reactions ; sadistic or masochistic impulses ; aggressive or sexual impulses ?

E. TRANSFERENCE AND COUNTER-TRANSFERENCE

(1) Do you analyse the transference situation only if it is so outstanding that it cannot be avoided or if it is a source of resistance, or do you regard it as the main therapeutic device ?

(2) How far do you think the personality of the analyst plays a part in the conduct of analysis ?

(3) Is the significance of social contact in analysis of secondary importance ?

(4) Do you desire to have any social contact with the patient after completion of the analysis ?

(5) What significance do you attach to the sex of the analyst ?

(6) In what cases would you prefer to recommend a male or female analyst ?

(7) Do you favour changing analysts for therapeutic purposes or changing to analyst of opposite sex ?

(8) Do you find the practice of analysis acts as a therapeutic procedure for the analyst or not : does it increase or diminish his own conflicts ?

F. PSYCHOTIC CASES

(1) Does your technique in such cases differ materially from your customary procedure ?

(2) Do you " reassure " more (see " B ") ?

(3) Are you more friendly ?

(4) Do you play super-ego roles ?

G. VARIOUS

(1) Do you favour analysing two or more members of the same family (at same or different times) ?

(2) Do you favour adopting any form of play technique in adult analysis (e.g. providing pencil and paper, etc.) ?

(3) Do you discuss the possible length of analysis during the first interview (transferred cases only) ?

(4) Do you frequently recommend a " trial trip " ?

APPENDIX II

SUMMARY OF REPORT.

A. POINTS ON WHICH THERE IS (ALMOST) COMPLETE AGREEMENT

1. Objection to use of technical terms.
2. Practice of analysing questions instead of answering them.
3. Objection to preliminary injunctions (apart from laying down association rule).
4. Transference analysis as general policy.
5. Avoidance of social (" fringe ") contact during analytical sessions : limitation of small talk.
6. Laying down rule of payment for non-attendance. (Rule not necessarily adhered to.)

B. GENERAL TENDENCIES

(representing a consensus of opinion among two-thirds of contributors : no correction for personal factors).

I. *With minority opinion up to one-third of the total but without pointed objection to general conclusion.*

1. In favour of acting as a source of information.
2. In favour of rapid short interpretations in acute anxiety, otherwise in favour of slow expansion.
3. Definition of reassurance as mitigation of anxiety by some means other than interpretation.
4. Attach special value to childhood memories.
5. Treat " common-sense " reactions of patient as reality defence.

6. Treat spontaneous offer of interpretation as transference manifestation.

7. Permit relaxation of association rule.

8. In favour of some form of " forced " phantasy.

9. Use of play technique in adult analysis sparing but not taboo.

10. Against any other " active " devices.

11. Regard transference analysis as main therapeutic device.

12. Favour neutrality of analyst.

13. Think patient reads analyst's unconscious but that transference " readings " are mostly projections.

14. Emphasise cooperative nature of analysis.

15. Allow telephone conversations, read letters, etc.

16. Accept small tokens from patients but no large or money gifts.

17. Make small loans (books) to patients.

18. Permit smoking but do not invite.

19. Keep same length of session and do not leave time-table gaps.

20. Do not see patients twice unless in emergency or as a special routine.

21. Against extra-analytical contact during analysis.

22. Reluctant to see relations, but see with patient's consent.

23. In favour of symptomatic criteria for termination (mostly where external factors interfere), otherwise three factors: symptomatic, psycho-sexual, and social.

24. Against trial trips and discontinuous analysis.

II. *With strongly expressed minority opinion, usually running sharply counter to the majority opinion.*

1. In favour of short compact interpretations.

2. In favour of interpretation in second half of session.

3. In favour of small amount during whole analysis.

4. Reassurance an emergency measure inevitable at some stage in every analysis: an adjunct, not an alternative to interpretation.

5. Allow time for working through.

6. Therapeutic effect of analysis upon analyst dominant.

7. Make admissions to patient if occasion arises but do not communicate personal opinions.

8. Define abstinence as self-imposed endurance of id-tension by the patient.

9. Against analysing members of family at same time.

10. Against analysing acquaintances or friends.

11. No great significance attaching to sex of analyst.

12. Against changing analysts during treatment.

C. POINTS ON WHICH THERE IS MARKED DIVISION OF OPINION (USUALLY EQUALLY DIVIDED)

1. Tracing themes (usually in any one session) in order to produce conviction.

2. Interpreting in the early part of any one session and sessional timing in general.

3. Problem of timing throughout analysis as a whole.

4. Amount and content during early phases: amount in terminal phases.

5. Opening up anxieties from different sources: order of interpretation (super-ego before id: sex before aggression: transference before unconscious content.)

6. Interpretation of symbols.

7. Reconstruction as an aid to memory recovery.

8. Type of case in which maximum memories recovered (hysteria a very small majority).

9. Methods of dealing with loquacity and with patients needing encouragement to talk.

10. The technique of " forced phantasy " : its value.

11. Restriction of spontaneity of analyst.

12. Professional as against personal friendliness to patients, shaking hands, small talk, the special case of the psychotic.

13. Smoking and non-smoking analysts about equal in number.

14. Clinical indications for male or female analyst.

15. Discussing length of analysis during first interview.

16. Criteria for termination.

17. Technique in psychotic cases.
18. The relation of theory to practice.
19. The etiological significance of aggressive and libidinal impulses respectively.
20. The etiological significance of different anxiety systems (genital, pre-genital, etc.).
21. The nature, content and timing of deep interpretation, with special reference to transference interpretation.

D. INDIVIDUAL OPINIONS

(strongly expressed).

1. Never use extraneous material in interpretation.
2. Never interpret early in session or early in analysis.
3. Never interpret at end of session or " round off."
4. Never be systematic in interpretation.
5. Never force interpretation from details.
6. Total quantity of anxiety more important than number of sources.
7. Everything is free association. What are relaxations ?
8. Not necessary to recommend even free association rule.
9. Analyse *all* transference manifestations, i.e. extra- as well as intra-analytic.
10. Negative transference analysis of supreme importance.
11. Communicate personal opinions.
12. Abstinence is a condition in which libidinal gratifications are sufficiently unobtainable to create a demand that stimulates the analytic work.
13. Women analysts better for both sexes.
14. Adopt mainly psycho-sexual criteria for termination.
15. Avoid guiding analysis by clinical criteria.
16. Use such criteria so far as they are dependable.
17. Adopt " infallibility " attitude in certain psychotic cases.
18. Make more social contact in psychotic cases.
19. Don't stress deep introjection-projection mechanisms in hysteria.

Although it is interesting to make a digest of the views expressed by all contributors, a certain correction

for error is necessary. To begin with, it should not be assumed that these are the only points of technique on which there is complete or general agreement. Neither of the Questionnaires pretended to be exhaustive. On the contrary, they dealt more with possible differences than with possible points of agreement. Hence it is not surprising to find evidence of considerable disagreement not only on detail but on principles. Similarly, it does not follow that the disagreements mentioned include all matters on which analysts hold divergent views.

With both these reservations the following conclusions may be drawn. It is quite clear that there is a general agreement in regarding analysis of the transference as the main therapeutic policy. There is also a general objection to " fringe " contacts and to the laying down of abstinence conditions. But the importance of transference analysis stands the test of detailed investigation better than the other general conclusions. It would appear that attitudes vary between comparatively rigid adherence to the formal analytic situation and a desire to modify it in a number of minor details which may convey to the patient the more human or humane aspects of the analyst's character. More sharp divergences begin to appear on the subject of interpretation. Apart from general agreement on the need for short and immediate interpretations in anxiety crises, there is wide variation on the technique of interpretation, its mode, length, timing, and depth. The " reassurance " problem is obviously unsettled and, in general, it may be concluded that the technique of interpretation is a much more individual matter than has previously been assumed. Unfortunately this con-

clusion, although encouraging enough to the student, is not entirely satisfactory. There are evidently some sharp cleavages which cannot be explained by minor individual differences in practice. It is significant that there is also a marked disagreement on questions of etiology, nature of anxiety, significance of aggressive impulses, termination, technique in psychoses, etc. These differences, taken together with the answers concerning the relation of theory to practice, suggest that preconceptions as to the importance of different developmental layers may influence radically the policy of interpretation.

So much for the more positive aspects of the report. The more negative aspects, the omissions to answer certain questions, the signs of hesitancy, diffidence, or timidity, the occasional apologetic explanations, the contradictions sometimes occurring in the same set of replies, the comparative over-emphasis of some isolated opinions, all these are as significant as the more positive findings. In a sense negative indications are more likely to be trustworthy. They are less subject to conventional reactions, to prestige suggestion, or to the need for conformity. The obvious disadvantage is that, although they indicate uncertainty on the part of the analyst, they do not give any reliable indication of his actual opinion on the question at issue. Examining the points which arouse the maximum amount of reservation, it is easy to see that most of them touch on the subject of counter-transference. The impression is difficult to avoid that there is a good deal of timidity in dealing with this aspect of the analytic situation. It seems to be taken for granted that the training analysis is sufficient correction for error in this direction. The

answers indicate that this is not the case. Indeed, it seems very probable that what has been described as " rigidity " in attitude, clinging to routine, particularly in the minutiæ of technique, is a safety device intended to arrest anxieties arising out of counter-transference reactions, either positive or negative.

Fortunately there is another side to this picture of indecision and uncertainty. Perhaps one of the most useful purposes served by these Questionnaires is to bring into the open comparatively wide differences in the application of analytic principles. It was pointed out in the introduction that in scientific meetings discussion is hampered by lack of time or systematisation, by timidity, and by prestige suggestion. The results of these Questionnaires indicate that, despite the monotony of publicly expressed views, there is a certain leaven of scientific interest and of individuality in approach in the British Society. This is a thoroughly satisfactory conclusion. It is important that the basic principles of psycho-analysis should be preserved, but this need not involve any sacrifice of elasticity in their application. " Closed " etiologies are not inevitably a sign of obscurantism ; they are precipitates of earlier clinical findings and serve as useful guiding posts to both student and practitioner. A " closed " system is obscurantist only when it is applied to the proliferating margin of research. With so many regions of psycho-pathology still uncharted, it is unreasonable to expect that new theories and formulations should have more than immediate empirical sanction. Regarded from this view-point the amount of uncertainty, confusion, and difference of opinion indicated in this report is to be welcomed. It is a proof that there is

yet no serious danger of psycho-analysis becoming dated and stultified by its own traditions.

It is not in the nature of every clinician to make new discoveries of outstanding importance, but it is essential that all analysts should regard themselves as research workers. Every case treated is in itself a " test " of theory and practice. Correlation of the results of such individual " tests " is the only way of ensuring the establishment and continued growth of sound principles and the elimination of error. In this work of verification every practitioner can and should take part. The wider the range of cooperation and the bigger the common " pool " of evidence, the higher the probability that subjective errors will cancel each other out and leave a core of objective validity. In no other branch of science is it more vital for the individual worker to retain his independence of mind, to prove and re-prove for himself and to form his judgments on the results within his own experience. So far psycho-analysis has successfully withstood attack from without and backsliding from within. Its framework has even benefited by these forms of erosion. There are only two conditions under which its efficiency as an instrument of research might become blunted, namely, the abandonment of empirical in favour of " perfectionist " criteria of theory or practice, and the (witting or unwitting) stimulation of anxiety or guilt factors, particularly in the younger generation of workers.

APPENDIX III

TECHNIQUE, 1934–1938.

IN PREPARING THIS Report some difficulty was experienced in deciding what amount of supplementary material should be included in the main text. Discussions of the Questionnaire were spread over a considerable period and, in the meantime, a certain number of these contributions were published as papers in the *International Journal*. For the sake of continuity it seemed desirable to include a summary of these in the body of the Report. Practically all references to work published before the Questionnaire were omitted. No doubt the value of the Report would have been considerably enhanced by including in each section a digest of earlier work relevant to it, but reasons of space and time precluded this course. The same objections do not apply to giving a summary of technical contributions that have been made since discussion of the Questionnaire died out. In the first place, these contributions have not been numerous and in the second it seemed worth while to examine later contributions in order to observe (*a*) whether discussion of the Questionnaire had brought about changes in attitude, or (*b*) whether the views expressed modified in any material way the main tendencies summarised in Appendix II. As has been noted, discussion of a number of points taking place subsequent to 1933

altered a good deal in character. Some earlier expressions of opinion were modified and to some extent the attitude of many members. During subsequent discussion of some less controversial points it became apparent that the attitude of these members was less formal and inelastic. At any rate, it became possible to raise such matters without mobilising attitudes of undue timidity or of excessive " correctness."

A. The Influence of Theory on Practice

On the other hand, the Society was soon confronted with an excellent illustration of the close connection between theory and practice (*see* Discussion, pp. 131–142). For an important technical issue developed out of the discussion of a paper which, although clinical in form, was concerned mainly with a new theoretical orientation in psycho-analysis.

In her paper entitled " A Contribution to the Psychogenesis of Manic Depressive States," * Melanie Klein stated that in her opinion " the infantile depressive position is the central position in the child's development." Starting with a brief re-statement of some of her earlier views, particularly concerning the phase of maximal sadism (which she believes to occur towards the end of the first year of life), the importance of introjection and projection of good or bad (part) objects and of the denial of psychic reality, she stated that a " depressive position " develops at the stage of passing from " part-object " to " whole-object " relations . . . " not till the object is loved *as a whole* can its loss be felt as a whole." At this point there is an increase in introjection processes in order that, amongst other reasons, the love-object may be preserved in safety inside oneself. There are, however, characteristic anxieties at this

* *I.J.P-A.*, 1935, vol. XVII, p. 174. Also a paper " Mourning and its relation to Manic-depressive States," October 16, 1938.

stage, in particular, " anxiety lest the object be destroyed in the process of introjection " and " as to the dangers which await the object inside." The situation that is " fundamental for the loss of the loved object " is when " the ego becomes fully identified with its good internalised objects and, at the same time, becomes aware of its own incapacity to protect and preserve them against the internalised persecuting objects and the id." The paranoid mechanisms of destroying objects (in particular " expulsion and projection ") persist, although in a lesser degree, but lose value because of the dread of expelling the *good* object along with the bad. " The ego makes greater use of introjection of the *good* object as a mechanism of defence. This is associated with another important mechanism, that of making reparation to the object." But " the ego cannot as yet believe enough in the benevolence of the object and in its own capacity to make restitution." Every access of hate or anxiety may temporarily abolish the differentiation between good and bad internal objects and this results in " loss of the loved object." The ego is " full of anxiety lest such objects should die." This represents a " disaster " caused by the child's sadism. In depression " the ego's hate of the id accounts even more for its unworthiness and despair than its reproaches against the object."

The paranoiac has also introjected a whole and real object, but has not been able to achieve a full identification with it. Sufferings associated with the depressive position may thrust him back on the paranoid position which can then be reinforced as a defence. The depressive state is genetically derived from the paranoid state. Another variety of defence is " manic " in type and is characterised by a sense of omnipotence, denial of psychic reality, and over-activity, all of which seek to deny the importance of the individual's good objects and to show contempt for them. The main aim of " manic defence " is to master and control all objects.

To sum up: The typical depressive phantasy might be crudely verbalised as follows — the good object is in pieces and cannot be put together again. In this connection although the child's relation to the father *imago* is referred to (particularly

in connection with sadistic phantasies of parental coitus, hostility to and from the combined parents, and restitution phantasies concerning both parents), study of the clinical and theoretical context suggests that in so far as one object is referred to it is more often than not the mother *imago*. It is, however, specifically stated that " From the beginning the ego introjects objects ' good ' and ' bad,' for both of which its mother's breast is the prototype."

As has been noted, the publication of this paper marked the commencement of a new orientation in psycho-analysis in a section of the British Society. The trend of discussions at subsequent meetings and the content of various papers soon indicated that a school of thought was developing based exclusively on a new hypothesis of development. Thus :

In the following year Joan Riviere * gave a paper in which she suggested that the " manic defence " may motivate the " negative therapeutic reaction." In this paper she accepted in their entirety Klein's ideas of the " depressive position " and of " manic defence." In a subsequent paper " On the genesis of psychical conflict in earliest infancy," † which was expanded from an " exchange " lecture given at Vienna, she endeavoured to establish a systematic metapsychological basis for the new views. Clinically, the most significant point in this paper was contained in a footnote where she committed herself to the explicit statement: " We have reason to think since Melanie Klein's latest work on depressive states that all neuroses are different varieties of defence against this fundamental anxiety, each embodying mechanisms which become increasingly available to the organism as its development proceeds."

The validity of Klein's views was accepted without reservation by several other members, e.g. in papers given to the

* " A Contribution to the Analysis of the Negative Therapeutic Reaction," *I.J.P-A.*, 1936, vol. XVI, p. 304.

† *I.J.P-A.*, 1936, vol. XVII, p. 395.

Society by Drs. Winnicott,* Rickman,† Scott,‡ Mrs. Isaacs,§ and Mrs. Rosenfeld,¶ and in various contributions to discussions by Drs. Heimann and Matthew.

Technical Implications.—Although apparently outwith the scope of a technical survey, these theoretical views have an important bearing on problems of technique. Thus, if the " depressive position " be indeed the central point of infantile development, no analysis can be considered adequate or, at any rate, complete, that fails to uncover and explore it. The first indications of this attitude were contained in Riviere's paper, where she suggested that in refractory cases of a narcissistic type : " (*a*) we should pay more attention to the analysis of the patient's inner world of object-relations, (*b*) that we should not be deceived by the positive aspects of his narcissism but should look deeper for the depression that will be found to underlie it." In course of time it became apparent that these technical recommendations were not to be confined to specially difficult cases. Discussion of every variety of clinical case in terms of " internal objects," " depressive positions," " manic defences," " reparation tendencies," etc., made it clear that a group or school of thought existed for whose members the analysis of

* D. W. Winnicott, " Manic Defence," December 4, 1935.

† J. Rickman, " A Study of Quaker Beliefs," June 3, 1935, and " The Nature of Ugliness," January 20, 1937.

‡ W. C. M. Scott, " Psycho-analysis of a Manic-depressive Patient in an Institution," June 2, 1937.

§ S. Isaacs, " An Acute Psychotic Anxiety occurring in a Boy of Four Years," February 2, 1938 ; " The Nature of the Evidence concerning Mental Life in the Earliest Years," April 6, 1938 ; " Temper-tantrums in Early Childhood in their relation to Internal Objects," December 7, 1938.

¶ E. Rosenfeld, " Psycho-analytic Approach in a Case of Psychosis," May 4, 1938.

these early anxieties and ego-defences, particularly in their transference form, constitute the main therapeutic aim of psycho-analysis.

The full effect of these views on the technique of psycho-analysis is not yet clear, but it is significant that already " preconscious " interpretations are regarded as relatively unimportant by members of this group. Moreover it is held that reality factors, whether occurring in childhood, in the current life-situation or in the transference, should not be assessed at face value but in terms of their " meaning " for the patient, which meaning is *a priori* an unconscious interpretation held to be made by the patient as the result of his early phantasies and anxieties. All of which implies that later events and development, including the classical Œdipus situation are, by comparison, unimportant. It is also claimed, openly or by implication, that better therapeutic results are obtained along the lines of interpretation indicated.

Criticism of the New Hypothesis.—The new formulations were not so readily accepted in the British Society as some of Melanie Klein's earlier work had been.* Thus :

In a discussion that took place on October 16, 1935, and on many subsequent occasions, Glover advanced a number of reasoned objections to her views, in particular the following : (*a*) that the building up of an " internalised-object-psychology " leads to confusion and obstruction instead of

* Or possibly some earlier uncertainties about her views came more definitely to expression. In a paper entitled " Some Aspects of Psycho-Analytical Research " (October 3, 1934), Glover reviewed outstanding problems in research and expressed the view that existing research activities were being " frozen " by the propagation of dogmatic views on matters concerning which a completely open mind was essential.

advancing existing concepts of early mental structure and function, e.g. confusion between " internalised objects " and id-instincts, between " projection " and " expulsion," between an " object-imago," an " introjection," and a " body-phantasy " ; * (*b*) that the " manic defence " and " depressive position " are neither clinical syndromes nor defence mechanisms, but a compound of already-established Freudian views with some inadequately substantiated theories ; that although the existence of depressive reactions, both symptomatic, and, in the case of the child, developmental, is beyond dispute, there is no justification for postulating a " central position " of this sort ; also that although the relations of clinical depression to clinical mania are also indisputable, it does not follow that there is a central and genetic sequence ; that manic defence is an arbitrarily constructed concept including such mechanisms as denial (*see* Freud and Abraham) which really belong to different phases or aspects of ego-development (c.f. repression mechanisms) ; that the " restitution " and " reparation " mechanisms associated with the " depressive position " are not organised until an obsessional phase that is clinically much later than that of depressive reactions ; that some of the " mechanisms " described are not defence-mechanisms but phantasies, e.g. danger of injuring the object in the act of introjection ; (*c*) that it has not yet been substantiated that analysts get better therapeutic results by basing their interpretations on the Klein hypotheses.

At the same time and on many subsequent occasions Melitta Schmideberg advanced similar criticisms, mainly : (*a*) that Melanie Klein's description of fixed sequences of psychotic positions is based on three assumptions, viz. the predominance of aggressive impulses, the predominance of projection and introjection mechanisms and the lack of reality sense in the infant. These assumptions have not, in M. Schmideberg's view, been adequately substantiated, and in any case the new theory involves a neglect of the importance of libido, of the

* In his paper on Introjection (*I.J.P-A.*, 1937, vol. XVIII, p. 269), Foulkes endeavoured to clear up the confusion arising out of various uses of this term.

effect of environmental factors in earliest infancy and of mechanisms like repression, isolation, conversion, sublimation, etc., which to some extent counteract projection and introjection ; (*b*) that one should distinguish between the frequency of clinical depression and the supposed theoretical importance of the " depressive position " ; (*c*) that a developmental etiology that leaves hysteria out of account and neglects schizophrenia cannot be regarded as satisfactory ; (*d*) that dynamic aspects of psychic situations are neglected ; (*e*) that some of the phantasies described are not primitive but of later origin, and in any case are frequently *distortions* of reality reactions and of more objective anxieties ; (*f*) that it is not satisfactory to explain clinical disturbances, e.g. paranoia, by displacing the symptom backwards into childhood and that there is no proof that the processes described actually take place in babyhood ; (*g*) that interpretations on the Klein model, by their very inexactitude, can act as reassurances, covering more preconscious worries and anxieties, by deflecting affect and criticism and encouraging flight into unreality (*see also* p. 94).

Environmental and Endopsychic Factors.—In all these discussions the issue of environmental factors in neuroso-genesis entered. One of the main criticisms directed against the Klein system was its neglect of reality, to which it was replied that this lack of emphasis was more apparent than real. It was maintained by supporters of the method that not only did they study the interplay of environmental and endopsychic factors, but that the only way of understanding the importance of reality factors is to see them as refracted through the child's early anxiety situations. The issue is obviously one of vital importance for the technique of interpretation, to say nothing of other practical matters such as diagnosis and prognosis. A long drawn out controversy developed during Society

discussions and a number of special papers were given on the subject. These discussions and papers are summarised below.

On various occasions M. Schmideberg maintained (*a*) that the assumption of rigid sequences of positions cannot make proper allowance for environmental factors, in other words, environmental factors are regarded as merely of quantitative, not of qualitative importance; (*b*) that reality factors are assessed in a tendentious way, that in particular there is displayed a bias in favour of the parents: the parents' self-valuation is accepted at face value; every event is interpreted on the assumption that the patient is guilty about it, and this gradually induces guilt in the patient; (*c*) that the neglect of reality factors and the stress laid on the " good mother " implies an idealisation of the mother-child relation and neglect of the ambivalence of both mother and child in more real levels.

The same author in a paper entitled " The Assessment of Environmental Factors " (February, 1936), stressed (*a*) the genetic importance of more continuous factors (in contrast to " traumatic " ones), operating even in the average or favourable environment; (*b*) the role of specific environmental factors in the first months of life; (*c*) the emotional attitude of the attendants, so often in contrast to their professed " modern " ideas; (*d*) that events repeatedly affecting derivatives of primitive instincts may exercise as marked an influence as those affecting the primary instincts themselves. She insisted that environmental factors should not be regarded in isolation, but always in the interplay of unconscious factors and mechanisms.

I. Matte Blanco read a paper on " A Case of Alcoholism " (June, 1938) (*see also* pp. 139–142) in which he stressed the importance, both for the structure of the case and for its treatment, of environmental factors in childhood, in particular the child's intuition concerning mother's real attitude, which contrasted with her professed kindness and understanding.

In this connection a paper of the late Dr. Middlemore,

" Observations of the Behaviour of Newborn Infants at the Breast " (read in November, 1937), is important. One of her conclusions was that " a different type of oral-sadistic phantasy will predominate where frustration is associated with over-activity of the mouth and with passivity respectively ; that the sensory state and mouth-behaviour during sucking will influence the development of feeling in other zones ; that early states of distress may be reactivated in later illness."

In a number of papers dealing with various aspects of sublimation Ella Sharpe emphasised the importance of infantile environmental factors (traumatic and other episodes or phases of influence) for sublimation processes.

The Nature of Evidence.—In these discussions questions of evidence and of verification of hypotheses inevitably arose. These focussed mainly on the assumptions made concerning mental processes in babyhood. These were considered in a series of " exchange " lectures between the British Society and the Vienna Society. Actually the exchange of views arose out of earlier differences concerning infantile development, but in practice it was limited to a discussion of the new hypotheses.

In a paper given before the British Society,* R. Wälder challenged the views of Klein and Riviere. This constituted the Vienna reply to Riviere's paper on " The Genesis of Psychic Conflict in Earliest Infancy."†

* " The Problem of the Genesis of Psychical Conflict in Earliest Infancy," *I.J.P-A.*, vol. XVIII, p. 406.

† *Loc. cit.* Interestingly enough, the English critics of Klein and her group did not associate themselves with Wälder, probably because they felt that he went too far in minimising everything that took place before the classical Œdipus situation, in particular the importance of pregenital factors. Although differing from the later Klein orientation, they did not object to some of her earlier findings which they felt could be woven into the texture of other analytic reconstructions, and which indeed were based to a large extent on accepted views of Freud and Abraham.

Partly in reply to Wälder's criticism Susan Isaacs gave a paper on " The Nature of Evidence concerning Mental Life in the Earliest Years " (April 6, 1938), concluding (from behaviouristic observations on babies and young children as well as from the analysis of older patients and children as young as 2½ years) that preverbal phantasies already existed in the first months of life. She stressed the view that sometimes actual early experiences could be inferred in the course of later analysis and supported her main argument in favour of drawing conclusions about babies from observations of older children by insisting on the principle of continuity. She also insisted that negative evidence should not be accepted as proof.

In the discussion M. Schmideberg objected to Isaacs' claims that negative evidence should be disallowed, and that there is no need to distinguish between analytic and behaviouristic observations. She maintained that Isaacs did not distinguish sufficiently between observations and the conclusions drawn from them ; and demanded that the principle of continuity should be supplemented by that of development, adding that, as we are not likely ever to have absolute certainty on what goes on in the mind of a baby under six months, we should avoid dogmatism.

Melitta Schmideberg, in a paper entitled " The Role of Suggestion in Psycho-Analytic Therapy "* (*see also* p. 94), put forward the view that suggestion plays a much greater part in analysis than analysts are willing to recognise, particularly by the subtler way of implication and inexact interpretation rather than by crude direct suggestion. She called for a fresh investigation of the whole situation in view of its practical and theoretical importance. To mention only one possibility, we may imagine at what distorted theoretical conclusions we should arrive if we mistook the results of implied suggestion for " uncovering the deepest layers of the unconscious."

Conclusion.—The inconclusive nature of this protracted discussion justifies the view expressed in the Preface to

* *The Psycho-Analytic Review*, vol. XXVI, April, 1939.

this Report, viz., that it is very desirable to make fresh investigations of technique from time to time. So long as these theoretical differences continue there is little prospect of standardising technique. And, as was suggested in the Introduction (p. 1), private discussions between small groups do not seem to help very much. On the contrary, they may even inflame differences. It cannot be denied that satisfactory progress in the theory and practice of psycho-analysis must depend on the investigation of infantile development prior to that phase familiarly labelled the phase of Classical Œdipus Conflict. But it is equally clear that to start separating the sheep from the goats on the strength of fresh hypotheses, however plausible, would be the very negation of scientific method. Empirical investigation of clinical detail is the only scientific course. It is true that the main onus of proof lies on those who advance new hypotheses that appear to run counter to accepted views. And it should be remembered that the main body of psycho-analytic findings has stood the test of protracted investigation. But, with these reservations, the situation calls for the mobilisation of non-partisan investigators who are ready to make advances without *neglecting or minimising the importance of already established work.*

B. Unclassified Papers on Technique

The following additional contributions are to be noted.

Melitta Schmideberg, in her paper " The Mode of Operation of Psycho-analytic Therapy,"* read at the Lucerne Congress, 1934, suggested that interpretations have a topographical, an economic and a dynamic effect.

* *I.J.P-A.*, 1938, vol. XIX, p. 310.

She regarded fusion and defusion, the formation of pre-conscious links, and the process of becoming conscious as more important than the retaining in consciousness of certain specific unconscious material, and stressed the view that the transference situation is a mixture of infantile situations and the reaction to the real personality of the analyst.

The same author, in a paper entitled " After the Analysis "* (*see also* p. 94), discussed the phantastic views (ideas of grandeur, denial of reality, resistance to analyses, over-compensation of anxiety and inferiority, etc.) expressed by some patients (and analysts) on what the patient will be like after having been " fully analysed," and contrasted the actual results achieved with these phantastic notions.

M. N. Searl, in her paper " Some Queries on Principles of Technique," † found herself in agreement with Hellmuth Kaiser ‡ " that the analysis of resistances should form practically the whole of our analytical work."

Miss Sheehan-Dare § stated: " A technique involving the minimum amount of activity on the part of the analyst is that best calculated to ensure the feeling of security essential to the working through of the patient's unconscious impulses and of his phantasies of omnipotence. In the interests of this aim reassurances should be avoided wherever possible. The analyst must be the rock against which the waves of the patient's affects can beat harmlessly *and in vain.* This opinion is illustrated by activities which turned out to be mistakes in technique:

" (*a*) An attempt to lessen guilt feeling in a male patient about his homosexual wishes by verbal reassurance to the effect that everyone was psychologically bisexual, instead of being felt as reassurance, became linked with unconscious phantasies and could clearly be seen to have increased repression.

" (*b*) Lending a small sum of money to a patient on a special

* *The Psycho-Analytic Review*, 1938, vol. VII, p. 122.
† *Ibid.*, 1936, vol. XVII, p. 471.
‡ Int. Ztschr. f. Pa. Bd. XX. S. 55.
§ " Technique in its relation to Phantasy."

occasion led to his immediate absence from analysis for a month. He had gradually acquired a feeling of security in the analysis because, up to that time, he had been unable to persuade me to lend him money. His success on this occasion increased his belief in his own phantasied omnipotence and its dangers, and seemed to confirm his suspicion that a hostile father figure was behind my action.

" Phantasies connected with paranoid anxieties in anti-social types frequently centre round a hidden person, supposedly in the background."

C. The Technique of Borderline and Psychotic Cases

In addition a number of papers having special bearing on the handling of psychotic and severe borderline cases were read. Author's abstracts of these are appended as follows.

In a paper read before the British Psycho-Analytical Society (June 5, 1935), Walter Schmideberg* reported the case of a girl of twenty who, behind the façade of a severe agoraphobia which for years had made it impossible for her to go out, suffered from schizophrenia. She was treated for three years daily and for two or three years less frequently. After one year's analysis her phobia disappeared and she was able to work; but simultaneously her delusions became more pronounced and she changed her occupation very frequently. At the end of the analysis her delusions had diminished in intensity, though they did not disappear entirely; she was well adapted, happier, and her street-phobia had vanished for good. Mr. Schmideberg remarks: " *Neither in this case nor in others did I find it necessary to modify my usual technique materially.* It is of the utmost importance never to lose contact with the patient, not even during the schizophrenic attack, and to continue interpreting during the attack. It is essential to be very careful in

* " Agoraphobia and Schizophrenia; a Contribution to the Analysis of Psychoses," June 5, 1935.

the handling of anxiety, especially the fear of going mad. This is achieved by reassurance as well as by interpretations, mainly by interpretations of the transference. It is also necessary to be careful with interpretations likely to stimulate anxiety and it is sometimes advisable to induce the patient to alter his environment during the analysis. I give 'deep interpretations' when they are justified, e.g. I interpret the delusion of being possessed by devils as fear of introjected objects, but on the whole I favour preconscious interpretations, especially of the transference.

"Using this technique I have analysed a number of cases of psychosis (schizophrenia, mania, and severe borderline cases). The patients' ages varied from nineteen to fifty years; the length of the treatment from three months to six years. In every case there was some improvement, in several cases a very far-reaching one, involving characterological changes, ability to work, and social adaptation, capacity for personal happiness, lessening of anxiety and disappearance (or lessening) of delusions. In no case was there a relapse after the analysis ended."

Summarising his experience of a variety of psychotic cases, Dr. Carroll* writes: "Psychotic patients do not form a homogeneous group. Consequently these remarks must not be taken as applying equally to all psychotic patients or even to all in one diagnostic group.

"The main clinical features involving technical modifications are: (*a*) rigidity and incapacity to tolerate anxiety; (*b*) wide variation in affective discharge; (*c*) defective appreciation of reality; (*d*) the narcissistic nature of the transference; (*e*) manifestations of the disease which may compel 'active' therapy.

"WHEN TO ANALYSE.—In general, whenever the patient is accessible. Nevertheless there seems to be no objection to attempting analysis during an attack, especially of depression in a manic depressive patient. Should it seem likely that interpretation will arouse intolerable anxiety and lead either

* "The Psycho-analytic Handling of Advanced Psychosis," June 17, 1936.

to increased severity of psychosis or to analysis being broken off, analytic treatment should be postponed—preferably to a remission. If analysed during an attack, such patients should be under supervision. This applies especially to manic and excited schizoid patients and agitated melancholics and those who have low tolerance for transference anxieties.

" CONDITIONS OF ANALYSIS.—The general rule is elasticity. I do not insist on the patient using the couch, lying down, not facing me, not walking about, etc. Many patients follow the classical arrangement without trouble. Paranoid (persecutory) types are the most resistant. Effect of insistence has been so bad, I prefer to err on the side of laxity.

" SUPERVISION.—Should be arranged on general psychiatric principles. It is possible to let matters go further than in a non-analytic situation, particularly if clinical manifestations developing are due to a transference situation which is well in hand (*see* SUICIDE). The disadvantages are due to the increase of negative transference and consequent lack of co-operation. Such reactions, however, do not necessarily follow even certification, so are no justification for timidity. As with the use of sedatives, it depends on whether the patient unconsciously identifies the proceeding with friendly or hostile measures. If history suggests that it is unlikely one can tolerate (or manage) patient under ordinary conditions, it is better to begin analysis under supervision whether necessary or not on clinical grounds. Except when such a step tends to identify the analyst with neglectful or other hostile figures, it may be better to let someone else arrange supervision and prescribe sedatives.

" SEDATIVES.—To be used on general principles. I have not found much advantage in withholding them. I often prescribe them myself, but would not do so if obsessional symptoms or ideas of poisoning (conscious or unconscious) were present in any strength.

" SUICIDE.—The following list of criteria for use in estimating risk is not complete.

" (a) *Danger Signals:* (1) Inadequately expressed and in-increasing unconscious or conscious anger (whether vengeful or not) towards analyst accompanied by increasing depression.

(2) Transference situation breaking down with tendency to break off analysis in (1). (3) Manifestations of increasing guilt with or without persecutory ideas about analyst—the guilt is mainly about aggressive impulses, but may only show its sexual side in early stages. (4) Lessening insight in (1), (2), or (3), or in other situation when suicide is thought of by patient. If appropriate interpretation fails and situation is not worsening or very severe I try sedatives. Otherwise, I use supervision and am frank about it. A comment which will lead to anger being displaced on to other people during the session is of use.

" (b) *Safety Signs :* (1) Positive transference strong in spite of negative manifestations. (2) Negative transference absorbing patient and making him still dependent on analyst provided insight is not lessening. (3) No tendency to break off. (4) Depression or persecutory ideas lessened by interpretation.

" TRANSFERENCE.—Though essentially narcissistic and intensely ambivalent, it is clinically similar to that in the neuroses. A typical transference neurosis has developed in my cases when they were accessible to analysis. Changes of sign and of affective discharge are often sudden and extreme and cause difficulty in handling. It is sometimes an advantage to share the handling (especially if it can be automatic as with a patient under institutional care) with another analyst or physician. I have done this with good results with a depressed patient with marked persecutory features who tended to produce unmanageable negative affects when aggressive impulses were first being analysed. The other physician kept the patient in analysis by obvious active and some interpretative devices. No bad effects on analysis and some good ones followed. The mechanism appears to be that an ambivalent patient is able to separate good and bad objects in terms of the two physicians.

" The analysis of the negative transference is the essential thing. I tend to make interpretations likely to arouse aggressive impulses or anxiety about them only if the positive transference seems strong enough to bear the strain. In general, I evoke positive transference in the early stages by means of interpretation. Positive transference develops following interpretation just as in the neuroses though, as a rule, it is slower and

weaker. The positive transference situation is unstable and I tend to great caution in dosage in making interpretations which may put a strain on it.

"COUNTER-TRANSFERENCE.—I find that quantities of negative counter-transference that would not matter with hysteria may cause a severe hold up and I am proportionately cautious. With patients who are difficult to handle I am inclined to believe a definite liking for the patient is essential if one is to reach any depth of analysis.

"INTERPRETATION.—Is made on same principles as in neuroses. Where the usual conversation technique seems inadequate, I tend to use any means of communication that offers, e.g. writing, drawing, playing, grimacing, acting, etc., especially where these means are used by the patient. Ultimately, as anxiety and inhibition lessen, I try to get the patient back to the classical routine. As regards length, frequency, mode of expression, tone of voice, etc., I have no special detailed practice for psychosis as opposed to neurosis, but the tendency is for all these factors to produce greater changes both negatively and positively than in neurotics. On the whole, I am less talkative and certainly less silent with psychotics.

"(a) *Emphasis and Repetition of Interpretation.*—On the whole, emphasis and repetition, if they do not succeed in making the interpretation effective, seem to frighten the patient. I would not press an interpretation if it were strongly resisted unless I felt certain such pressure would succeed and even then I would hesitate with a negativistic or very egocentric patient.

"(b) *Type of Interpretation.*—On the whole, I use 'deep' and 'superficial' interpretation as in neurosis. In general, this means superficial interpretation in the early stages and deep later on. In some cases of true paranoia and schizophrenia and one manic case I found that the patient thinks so much in archaic patterns that one can sometimes make deep interpretations almost at once with good effect. They must be disguised enough for acceptance and a slip in finding out how to disguise them does not seem to matter. They do not have the effect of arousing in consciousness the appropriate quantity of affect and I doubt if they achieve more in this respect than so-called

superficial interpretation. In the types mentioned I have sometimes been able to resolve difficulties of early negative transference by deep though not by superficial interpretation, e.g. in the third week of analysis of a true paranoia I was able to do this by referring direct to oral aggressive impulses directed towards my bodily contents regarded as powerful objects made bad by projection (*see* PROJECTION). This is a rare experience with me at such an early stage. Some patients, especially schizophrenics and manics, will turn deep interpretation into a futile game.

"(c) *Special Types of Interpretation.*—(1) Aggression. I tend to interpret the aggressive significance of the associations early and, other things being equal, to continue these interpretations. It seems important not to overdo this and to avoid interpretation appearing accusatory, especially in depressives who may become suicidal if such misplaced or misexpressed interpretations are pressed. A slight increase of depression need not stop this process, nor need an attempt to hide it behind apparent sexual guilt or sexual transference difficulties do so, but in any case I try to give doses of such interpretation between other more or less relevant kinds and give ample time for working through afterwards. In the later stages, too, the reaction may be volcanic but the difficulties seem less, probably because the manifestations are more limited to the analytic situation and because relief by further interpretation can be more rapidly given.

"(2) Projection. I have noticed that in patients who are predominantly paranoid there is an advantage in the early and direct interpretation of projection. Enough detail to demonstrate the correctness of the interpretation is required and the time to give the interpretation is when the projection is of such a kind that it blocks the analysis by colouring the transference situation and the block is in danger of becoming absolute.

"(d) *Delusional and Hallucinatory Material.*—I accept the patient's view and give interpretations in the same way as I would about real experiences. As a rule, the effect of analysis is indirect, though I once saw a delusion disappear suddenly following interpretations of the affective content in terms of ideas about introjected objects on the tenth day of analysis.

The affects released were considerable, but the patient certainly did not get any insight into her unconscious ideas about introjection. It is a very rare example. If this material is frightening, etc., I am quite willing to appear actively as a protector, e.g. by arranging supervision.

"TERMINAL STAGE.—I have not had many opportunities of deliberately arranging one. In the first instance, I follow Freud's suggestions for terminal stages in neuroses. It has caused severe reactions if suggested too early, and I approach the subject very tentatively. About nine months or a year seems long enough for a terminal stage provided it be continuous. Analysis has usually ended because I have done all I could for the time being and I have sometimes stopped a long analysis quickly at the patient's suggestion without a terminal stage. I make considerable use of intermittent analysis over a longer period as a method of stopping. It seems to permit of more analysis of aggression than is possible with the classical continuous terminal stage. Indeed, some psychotic patients enforce an intermittent pattern in analysis as a whole.

"ACTIVE MEASURES.—These may be involved as described above. On the whole, the less the patient is in touch with reality the less the difficulty produced in subsequent analysis. I use 'active therapy' (Ferenczi, etc.) but little and on the same principles as apply to neurotics. There are exceptions:

"(1) *Relaxation.*—This is definitely of more frequent value than in neurosis.

"(2) *Education.*—I give guidance about real life crises when the patient is unable to cope with them because of his psychosis or childishness. I wait until he realises his danger to some extent and make a point of interpreting any probable tendency in him to compulsive restaging of the situation in order to manipulate the transference situation. With adolescent patients whose experience of normal life has been severely limited by the disease I have given help in emotional problems."

Dr. Scott* summarises his views on the technique of such cases as follows:

* *Op. cit.*, p. 163.

" I think that the chief differences between my work with psychotic patients and neurotic patients are :

" (1) In formulating the free-association rule I do not stress my wish that the patient lie so that he cannot see me—and I make it equally easy for the patient to sit as to lie.

" (2) I do not hesitate so much to use what the patient brings to the analysis in the form of writing or drawings—but I try to bring them into the analysis as soon as possible by asking him to read what he has written—or to let me read it aloud—and as far as drawings are concerned I try to get him to tell me what he has drawn.

" (3) I find the analysis of psychotic patients more like that of those children I have analysed in so far as their activity in analysis has always been greater (except for periods during which they may be abnormally inactive) and I have used activity as the basis of interpretation more frequently than with other patients. An instance of this activity was in a patient who wished the room to be dark—this was allowed, but I kept control of the light.

" (4) I have found that it has been helpful to let psychotics have a drink of water during the analysis at times—and I have given several severely psychotic patients a sweet at a time when oral aggression or libidinous desire was great. (I have kept this in mind since a psychotic found and took some barley sugar I had in my office with what I came to think were good results as far as the analysis was concerned.)

" (5) I do not find that I reassure more (here I am not clear as to the meaning of reassurance—but in so far as it means either telling the patients something about external reality which one thinks they do not know and which would help them if they did know—or in so far as it means assuming an omnipotent role with regard to foretelling the future, the above holds true, I think).

" (6) I do not think I am more friendly on the whole (the exceptions have been with physicians whom I have analysed—I have not refused at times to discuss for a short time a medical problem)."

Eva Rosenfeld* describes her technique in the analysis of a depressive psychosis in a girl of eighteen years, as follows :

" Both the apparent lack of transference (e.g. continued silence) and violent anxieties are interpreted in the fullest detail. The more acute the patient's anxiety, the more urgent the need for interpretation. Interpretations require to be *complete.* A complete interpretation begins with the patient's relation to his internal objects, and includes the analysis of his anxiety, depression, and guilt, due to his aggression, triumph over his injured object, and consequent persecution phantasies. When these anxieties have been lessened, then the raving of his cruel super-ego has been lessened at the same time. This allows the appearance of the forbidden impulses in an open transference setting, but this will lead to new and extremely violent anxieties. This open aggression and anxiety is a dangerous but not an undesirable state. Violent emotional outbursts are sometimes inevitable, but if fully interpreted prove to be turning points of improvement, since it was the fear of such outbursts (infantile temper tantrums) which led originally to the repression, and the fear of their consequences (loss of the loved object) led to the narcissistic defence. The work of interpretation has to go on during the outbursts of violence. The fact that the analyst is not too frightened or too angry to deal with these outbursts makes reality more tolerable."

Dr. M. Schmideberg† writes concerning the analysis of a severe borderline case : " This girl of twenty, before she came to me, had made six serious suicidal attempts by poisoning. She came to see me immediately after the last attempt and begged me in despair not to send her to a nursing-home. I put her up in my house, where she remained semi-conscious under drugs for a week before I could get in touch with her relations. Then arrangements were made for her to live with a nurse under strict supervision. In spite of this, she made a second attempt one month later. The next difficulty was her

* *Op. cit.*, p. 163.

† Technical Problems in a Suicidal Case (May 18, 1938).

almost complete refusal to eat for some months. When the loss of weight became dangerous I had to call in a physician, who induced her to eat by means of suggestion. Soon she relapsed again and starved herself for a further year till organic complications set in. She had a phobia of most people, including her parents, and in emotional emergencies I had to look after her a good deal, e.g. put her up for the night. She gradually became able, though with great difficulty, to carry on some work which I found for her. After about eighteen months of analysis I had to stop owing to strong external pressure brought to bear on her. Since then the girl has lived with her parents, led a normal and seemingly happy life, and after a time she married.

"Apart from the danger of suicide, the main technical obstacle was her extreme difficulty in making contact. She suffered from emotional rigidity and severe depersonalisation interrupted only occasionally by violent anxiety and despair. She had very great difficulties in speaking. These difficulties were increased by the parents' attitude, which was fundamentally very antagonistic. As it was a life and death matter I had no choice but to analyse in the way I have described. I did so without interruption even when she was under the influence of drugs, alternating this with reassurance and comforting. Though on the one hand I was for her a person in authority who insisted on strict supervision and protected her against her parents, on the other I was more motherly than to any other patient, e.g. when she was in complete despair I would put my arms round her as if she were a child. Although this was in many respects an unusual case calling for extremely elastic handling, the interesting technical point is that I found it possible to combine the functions of a foster-mother, of a supervisor (and on occasion of a detective) with those of an analyst. That analysis could be carried out in such unique circumstances, was due, I believe, (*a*) to the fact that she felt and appreciated my attitude, (*b*) that I tried throughout to analyse the underlying negative transference."

Dr. Matte* writes: "This was a case of alcoholism, aged

* *Op. cit.*, p. 167.

38 years. The habit had persisted from adolescence. He drank heavily and only in the last few years before analysis did he fight against it. He succeeded in having a few abstemious periods, the longest of which lasted, I believe, 13 months. After this he drank increasingly heavily and just before analysis commenced was in a state of collapse.

"The case responded to the classical procedures of analysis except in one respect, viz., the actual handling of the alcohol situation. He was analysed in the ordinary way, and after a few months felt better. As on earlier occasions of abstinence, he began to react to alcohol as if it were a 'taboo' substance. At this point I deviated from the conventional practice employed with psycho-neurotics by pointing out that in view of his taboo reactions I would not consider his condition satisfactory unless he could drink without going to excess. After a period during which these points were exhaustively considered in the analysis, he began to drink a little and gradually increased the dose. About the same time he developed a variety of psycho-pathological symptoms. After persistent analysis he got to the stage when he could drink quite heavily without, as before, ending up with complete collapse. Not only did he control his drinking, but for the first time in his life he was able to continue at work. Thereafter the drinking diminished up to the point when the analysis broke off owing to extrinsic causes. He was improved, though far from cured.

"Amongst many incidents concerned with his drinking reactions, the following is significant. On one occasion, at the end of the last session, before a holiday, he expressed the desire that I should have a drink with him at a public-house in the vicinity. Although familiar with the various unconscious components of his wish, I decided to accept the invitation and I had good reason to believe that this gesture enabled him to avoid a drinking bout during that holiday. I have no doubt there are many unusual situations consequent on heavy drinking habits which may call for elasticity of handling on the part of the analyst."

The only possible comment on these contributions

is that they indicate the necessity for further investigation of the subject, in particular for classification of those disorders in which modifications of the ordinary analytic technique are not only inevitable but desirable. We must remember that analysts usually base their statements on the technique in psychotic cases on a comparison with that used in cases of psycho-neurosis. But the latter, as we have seen, is by no means so fixed as is generally supposed. To anyone with extensive clinical experience the wide variations in technical approach are not surprising. There are many psychotic types who are comparatively easy to approach, using the classical technique. On the other hand, no one who has attempted the analysis of " transitional " groups (lying between the neuroses and psychoses), e.g. semi-perverted drug addicts with depressive or violent phases, can hope to make any progress unless he is prepared to put up with the greatest personal inconvenience, a good deal of professional risk, and the most surprising demands on his powers of technical adaptation.

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